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CHANCE AND LUCK

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CHANCE AND LUCK:

A DISCUSSION OF

THE LAWS OF LUCK, COINCIDENCES,

WAGERS, LOTTERIES, AND THE FALLACIES OF GAMBLING;

WITH NOTES ON

POKER AND MARTINGALES.

BY

RICHARD A. PROCTOR,

AUTHOR OF 'HOW TO PLAY WHIST,' 'HOME WHIST,' 'EASY LESSONS IN THE DIFFERENTIAL CALCULUS,' AND THE ARTICLES ON ASTRONOMY IN THE 'ENCYCLOPÆDIA BRITANNICA' AND THE 'AMERICAN CYCLOPÆDIA.'

'Looking before and after.'—*Shakespeare.*

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PREFACE.

THE false ideas prevalent among all classes of the community, cultured as well as uncultured, respecting chance and luck, illustrate the truth that common consent (in matters outside the influence of authority) argues almost of necessity *error*. This, by the way, might be proved by the method of probabilities. For if, in any question of difficulty, the chance that an average mind will miss the correct opinion is but one-half—and this is much underrating the chance of error—the probability that the larger proportion of a community numbering many millions will judge rightly on any such question is but as one in many millions of millions of millions. (Those who are too ready to appeal to the argument from common consent, and on the strength of it sometimes to denounce or even afflict their fellow men, should take this fact—for it is fact, not opinion—very thoughtfully to heart.)

I cannot hope, then, since authority has never been at the pains to pronounce definitely on such questions respecting luck and chance as are dealt with here, that common opinion, which is proclaimed constantly and

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loudly in favour of faith in luck, will readily accept the teachings I have advanced, though they be but the commonplace of science in regard to the dependence of what is commonly called *luck*, strictly, and in the long run, uniformly, on *law*. The gambling fraternity will continue to proclaim their belief in luck (though those who have proved successful among them have by no means trusted to it), and the community on whom they prey will, for the most part, continue to submit to the process of plucking, in full belief that they are on their way to fortune.

If a few shall be taught, by what I have explained here, to see that in the long run even fair wagering and gambling must lead to loss, while gambling and wagering scarcely ever are fair, in the sense of being on even terms, this book will have served a useful purpose. I wish I could hope that it would serve the higher purpose of showing that all forms of gambling and speculation are essentially immoral, and that, though many who gamble are not consciously wrongdoers, their very unconsciousness of evil indicates an uncultured, semi-savage mind.

RICHARD A. PROCTOR.

Saint Joseph, Mo. 1887.

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CHANCE AND LUCK.

LAWS OF LUCK.

To the student of science, accustomed to recognise the operation of law in all phenomena, even though the nature of the law and the manner of its operation may be unknown, there is something strange in the prevalent belief in luck. In the operations of nature and in the actions of men, in commercial transactions and in chance games, the great majority of men recognise the prevalence of something outside law—the good fortune or the bad fortune of men or of nations, the luckiness or unluckiness of special times and seasons—in fine (though they would hardly admit as much in words), the influence of something extranatural if not supernatural. [For to the man of science, in his work as student of nature, the word 'natural' implies the action of law, and the occurrence of aught depending on what men mean by luck would be simply the occurrence of something supernatural.] This is true alike of great things and of small; of matters having a certain dignity, real or apparent, and of matters which seem utterly con-

temptible. Napoleon announcing that a certain star (as he supposed) seen in full daylight was *his* star and indicated at the moment the ascendency of his fortune, or William the Conqueror proclaiming, as he rose with hands full of earth from his accidental fall on the Sussex shore, that he was destined by fate to seize England, may not seem comparable with a gambler who says that he shall win because he is in the vein, or with a player at whist who rejoices that the cards he and his partner use are of a particular colour, or expects a change from bad to good luck because he has turned his chair round thrice; but one and all are alike absurd in the eyes of the student of science, who sees law, and not luck, in all things that happen. He knows that Napoleon's imagined star was the planet Venus, bound to be where Napoleon and his officers saw it by laws which it had followed for past millions of years, and will doubtless follow for millions of years to come. He knows that William fell (if by accident at all) because of certain natural conditions affecting him physiologically (probably he was excited and over anxious) and physically, not by any influence affecting him extranaturally. But he sees equally well that the gambler's superstitions about 'the vein,' the 'maturity of the chances,' about luck and about change of luck, relate to matters which are not only subject to law, but may be dealt with by processes of calculation. He recognises even in men's belief in luck the action of law, and in the use which clever men like Napoleon and William have made of this false faith of men in luck, a natural result of cerebral

development, of inherited qualities, and of the system of training which such credulous folk have passed through.

Let us consider, however, the general idea which most men have respecting what they call luck. We shall find that what they regard as affording clear evidence that there is such a thing as luck is in reality the result of law. Nay, they adopt such a combination of ideas about events which seem fortuitous that the kind of evidence they obtain must have been obtained, let events fall as they may.

Let us consider the ideas of men about luck in gambling, as typifying in small the ideas of nearly all men about luck in life.

In the first place, gamblers recognise some men as always lucky. I do not mean, of course, that they suppose some men always win, but that some men never have spells of bad luck. They are *always* 'in the vein,' to use the phraseology of gamblers like Steinmetz and others, who imagine that they have reduced their wild and wandering notions about luck into a science.

Next, gamblers recognise those who start on a gambling career with singular good luck, retaining that luck long enough to learn to trust in it confidently, and then losing it once for all, remaining thereafter constantly unlucky.

Thirdly, gamblers regard the great bulk of their community as men of varying luck—sometimes in the 'vein' sometimes not—men who, if they are to be successful, must, according to the superstitions of the

gambling world, be most careful to watch the progress of events. These, according to Steinmetz, the great authority on all such questions (probably because of the earnestness of his belief in gambling superstitions), may gamble or not, according as they are ready or not to obey the dictates of gambling prudence. When they are in the vein they should gamble steadily on; but so soon as 'the maturity of the chances' brings with it a change of luck they must withdraw. If they will not do this they are likely to join the crew of the unlucky.

Fourthly, there are those, according to the ideas of gamblers, who are pursued by constant ill-luck. They are never 'in the vein.' If they win during the first half of an evening, they lose more during the latter half. But usually they lose all the time.

Fifthly, gamblers recognise a class who, having begun unfortunately, have had a change of luck later, and have become members of the lucky fraternity. This change they usually ascribe to some action or event which, to the less brilliant imaginations of outsiders, would seem to have nothing whatever to do with the gambler's luck. For instance, the luck changed when the man married—his wife being a shrew; or because he took to wearing white waistcoats; or because so-and-so, who had been a sort of evil genius to the unlucky man, had gone abroad or died; or for some equally preposterous reason.

Then there are special classes of lucky or unlucky men, or special peculiarities of luck, believed in by individual gamblers, but not generally recognised.

Thus there are some who believe that they are lucky on certain days of the week, and unlucky on certain other days. The skilful whist-player who, under the name 'Pembridge,' deplores the rise of the system of signals in whist play, believes that he is lucky for a spell of five years, unlucky for the next five years, and so on continually. Bulwer Lytton believed that he always lost at whist when a certain man was at the same table, or in the same room, or even in the same house. And there are other cases equally absurd.

Now, at the outset, it is to be remarked that, if any large number of persons set to work at any form of gambling—card play, racing, or whatever else it may be—their fortunes *must* be such, let the individual members of the company be whom they may, that they will be divisible into such sets as are indicated above. If the numbers are only large enough, not one of those classes, not even the special classes mentioned at the last, can fail to be represented.

Consider, for instance, the following simple illustrative case:—

Suppose a large number of persons—say, for instance, twenty millions—engage in some game depending wholly on chance, two persons taking part in each game, so that there are ten million contests. Now, it is obvious that, whether the chances in each contest are exactly equal or not, exactly ten millions of the twenty millions of persons will rise up winners and as many will rise up losers, the game being understood to be of such a kind that one player or the other must win. So

far, then, as the results of that first set of contests are concerned, there will be ten million persons who will consider themselves to be in luck.

Now, let the same twenty millions of persons engage a second time in the same two-handed game, the pairs of players being not the same as at the first encounter, but distributed as chance may direct. Then there will be ten millions of winners and ten millions of losers. Again, if we consider the fortunes of the ten million winners on the first night, we see that, since the chance which each one of these has of being again a winner is equal to the chance he has of losing, *about* one-half of the winning ten millions of the first night will be winners on the second night too. Nor shall we deduce a wrong general result if, for convenience, we say *exactly* one-half; so long as we are dealing with very large numbers we know that this result must be near the truth, and in chance problems of this sort we require (and can expect) no more. On this assumption, there are at the end of the second contest five millions who have won in both encounters, and five millions who have won in the first and lost in the second. The other ten millions, who lost in the first encounter, may similarly be divided into five millions who lost also in the second, and as many who won in the second. Thus, at the end of the second encounter, there are five millions of players who deem themselves lucky, as they have won twice and not lost at all; as many who deem themselves unlucky, having lost in both encounters; while ten millions, or half the original number, have no reason

to regard themselves as either lucky or unlucky, having won and lost in equal degree.

Extending our investigation to a third contest, we find that 2,500,000 will be confirmed in their opinion that they are very lucky, since they will have won in all three encounters; while as many will have lost in all three, and begin to regard themselves, and to be regarded by their fellow-gamblers, as hopelessly unlucky. Of the remaining fifteen millions of players, it will be found that 7,500,000 will have won twice and lost once, while as many will have lost twice and won once. (There will be 2,500,000 who won the first two games and lost the third, as many who lost the first two and won the third, as many who won the first, lost the second, and won the third, and so on through the six possible results for these fifteen millions who had mixed luck.) Half of the fifteen millions will deem themselves rather lucky, while the other half will deem themselves rather unlucky. None, of course, can have had even luck, since an odd number of games has been played.

Our 20,000,000 players enter on a fourth series of encounters. At its close there are found to be 1,250,000 very lucky players, who have won in all four encounters, and as many unlucky ones who have lost in all four. Of the 2,500,000 players who had won in three encounters, one-half lose in the fourth; they had been deemed lucky, but now their luck has changed. So with the 2,500,000 who had been thus far unlucky: one-half of them win on the fourth trial. We have

then 1,250,000 winners of three games out of four, and 1,250,000 losers of three games out of four. Of the 7,500,000 who had won two and lost one, one-half, or 3,750,000, win another game, and must be added to the 1,250,000 just mentioned, making three million winners of three games out of four. The other half lose the fourth game, giving us 3,750,000 who have had equal fortunes thus far, winning two games and losing two. Of the other 7,500,000, who had lost two and won one, half win the fourth game, and so give 3,750,000 more who have lost two games and won two: thus in all we have 7,500,000 who have had equal fortunes. The others lose at the fourth trial, and give us 3,500,000 to be added to the 1,250,000 already counted, who have lost thrice and won once only.

At the close, then, of the fourth encounter, we find a million and a quarter of players who have been constantly lucky, and as many who have been constantly unlucky. Five millions, having won three games out of four, consider themselves to have better luck than the average; while as many, having lost three games out of four, regard themselves as unlucky. Lastly, we have seven millions and a half who have won and lost in equal degree. These, it will be seen, constitute the largest part of our gambling community, though not equal to the other classes taken together. They are, in fact, three-eighths of the entire community.

So we might proceed to consider the twenty millions of gamblers after a fifth encounter, a sixth, and so on. Nor is there any difficulty in dealing with the

matter in that way. But a sort of account must be kept in proceeding from the various classes considered in dealing with the fourth encounter to those resulting from the fifth, from these to those resulting from the sixth, and so on. And although the accounts thus requiring to be drawn up are easily dealt with, the little sums (in division by two, and in addition) would not present an appearance suited to these pages. I therefore now proceed to consider only the results, or rather such of the results as bear most upon my subject.

After the fifth encounter there would be (on the assumption of results being always exactly balanced, which is convenient, and quite near enough to the truth for our present purpose) 625,000 persons who would have won every game they had played, and as many who had lost every game. These would represent the persistently lucky and unlucky men of our gambling community. There would be 625,000 who, having won four times in succession, now lost, and as many who, having lost four times in succession, now won. These would be the examples of luck—good or bad—continued to a certain stage, and then changing. The balance of our 20,000,000, amounting to seventeen millions and a half, would have had varying degrees of luck, from those who had won four games (not the first four) and lost one, to those who had lost four games (not the first four) and won but a single game. The bulk of the seventeen millions and a half would include those who would have had no reason to regard themselves as either specially lucky or

specially unlucky. But 1,250,000 of them would be regarded as examples of a change of luck, being 625,000 who had won the first three games and lost the remaining two, and as many who had lost the first three games and won the last two.

Thus, after the fifth game, there would be only 1,250,000 of those regarded (for the nonce) as persistently lucky or unlucky (as many of one class as of the other), while there would be twice as many who would be regarded by those who knew of their fortunes, and of course by themselves, as examples of change of luck, marked good or bad luck at starting, and then bad or good luck.

So the games would proceed, half of the persistently lucky up to a given game going out of that class at the next game to become examples of a change of luck, so that the number of the persistently lucky would rapidly diminish as the play continued. So would the number of the persistently unlucky continually diminish, half going out at each new encounter to join the ranks of those who had long been unlucky, but had at last experienced a change of fortune.

After the twentieth game, if we suppose constant exact halving to take place as far as possible, and then to be followed by halving as near as possible, there would be about a score who had won every game of the twenty. No amount of reasoning would persuade these players, or those who had heard of their fortunes, that they were not exceedingly lucky persons—not in the sense of being lucky because they *had* won, but of

being *likelier to win* at any time than any of those who had taken part in the twenty games. They themselves and their friends—ay, and their enemies too—would conclude that they '*could* not lose.' In like manner, the score or so who had not won a single game out of the twenty would be judged to be most unlucky persons, whom it would be madness to back in any matter of pure chance.

Yet—to pause for a moment on the case of these apparently most manifest examples of persistent luck—the result we have obtained has been to show that inevitably there must be in a given number of trials about a score of these cases of persistent luck, good or bad, and about two score of cases where both good and bad are counted together. We have shown that, without imagining any antecedent luckiness, good or bad, there *must* be what, to the players themselves, and to all who heard of or saw what had happened to them, would seem examples of the most marvellous luck. Supposing, as we have, that the game is one of pure chance, so that skill cannot influence it and cheating is wholly prevented, all betting men would be disposed to say, 'These twenty are persons whose good luck can be depended on; we must certainly back them for the next game: and those other twenty are hopelessly unlucky; we may lay almost any odds against their winning.'

But it should hardly be necessary to say that that which *must* happen cannot be regarded as due to luck. There must be *some* set of twenty or so out of our twenty millions who will win every game of twenty; and the

circumstance that this has befallen such and such persons no more means that they are lucky, and is no more a matter to be marvelled at, than the circumstance that one person has drawn the prize ticket out of twenty at a lottery is marvellous, or signifies that he would be always lucky in lottery drawing.

The question whether those twenty persons who had so far been persistently lucky would be better worth backing than the rest of the twenty millions, and especially than the other twenty who had persistently lost, would in reality be disposed of at the twenty-first trial in a very decisive way: for of the former score about half would lose, while of the latter score about half would win. Among a thousand persons who had backed the former set at odds there would be a heavy average of loss; and the like among a thousand persons who had laid against the latter set at odds.

It may be said this is assertion only, that experience shows that some men are lucky and others unlucky at games or other matters depending purely on chance, and it must be safer to back the former and to wager against the latter. The answer is that the matter has been tested over and over again by experience, with the result that, as *à priori* reasoning had shown, some men are bound to be fortunate again and again in any great number of trials, but that these are no more likely to be fortunate on fresh trials than others, including those who have been most unfortunate. The success of the former shows only that they *have been*, not that they

are lucky; while the failure of the others shows that they *have* failed, nothing more.

An objection will—about here—have vaguely presented itself to believers in luck, viz. that, according to the doctrine of the 'maturity of the chances,' which must apply to the fortunes of individuals as well as to the turn of events, one would rather expect the twenty who had been so persistently lucky to lose on the twenty-first trial, and the twenty who had lost so long to win at last in that event. Of course, if gambling superstitions might equally lead men to expect a change of luck and continuance of luck unchanged, one or other view might fairly be expected to be confirmed by events. And on a single trial one or other event—that is, a win or a loss—*must* come off, greatly to the gratification of believers in luck. In one case they could say, 'I told you so, such luck as A's was bound to pull him through again'; in the other, 'I told you so, such luck was bound to change': or if it were the loser of twenty trials who was in question, then, 'I told you so, he was bound to win at last'; or, 'I told you so, such an unlucky fellow was bound to lose.' But unfortunately, though the believers in luck thus run with the hare and hunt with the hounds, though they are prepared to find any and every event confirming their notions about luck, yet when a score of trials or so are made, as in our supposed case of a twenty-first game, the chances are that they would be contradicted by the event. The twenty constant winners would not be more lucky than the twenty constant losers; but neither would they be less

lucky. The chances are that about half would win and about half would lose. If one who really understands the laws of probability could be supposed foolish enough to wager money on either twenty, or on both, he would unquestionably regard the betting as perfectly even.

Let us return to the rest of our twenty millions of players, though we need by no means consider all the various classes into which they may be divided, for the number of these classes amounts, in fact, to more than a million.

The great bulk of the twenty millions would consist of players who had won about as many games as they had lost. The number who had won *exactly* as many games as they had lost would no longer form a large proportion of the total, though it would form the largest individual class. There would be nearly 3,700,000 of these, while there would be about 3,400,000 who had won eleven and lost nine, and as many who had won nine and lost eleven; these two classes together would outnumber the winners of ten games exactly, in the proportion of 20 to 11 or thereabouts. Speaking generally, it may be said that about two-thirds of the community would consider they had had neither good luck nor bad, though their opinion would depend on temperament in part. For some men are more sensitive to losses than to gains, and are ready to speak of themselves as unlucky, when a careful examination of their varying fortunes shows that they have neither won nor lost on the whole, or have won rather more than they have lost. On the

other hand, there are some who are more exhilarated by success than dashed by failure.

The number of those who, having begun with good luck, had eventually been so markedly unfortunate, would be considerable. It might be taken to include all who had won the first six games and lost all the rest, or who had won the first seven or the first eight, or any number up to, say, the first fourteen, losing thence to the end; and so estimated would amount to about 170, an equal number being first markedly unfortunate, and then constantly fortunate. But the number who had experienced a marked change of luck would be much greater if it were taken to include all who had won a large proportion of the first nine or ten games and lost a large proportion of the remainder, or *vice versâ*. These two classes of players would be well represented.

Thus, then, we see that, setting enough persons playing at any game of pure chance, and assuming only that among any large number of players there will be about as many winners as losers, irrespective of luck, good or bad, all the five classes which gambling folk recognise and regard as proving the existence of luck, must inevitably make their appearance.

Even any special class which some believer in luck, who was more or less fanciful, imagined he had recognised among gambling folk, must inevitably appear among our twenty millions of illustrative players. For example, there would be about a score of players who would have won the first game, lost the second, won

the third, and so on alternately to the end; and as many who had also won and lost alternate games, but had lost the first game; some forty, therefore, whose fortune it seemed to be to win only after they had lost and to lose only after they had won. Again, about twenty would win the first five games, lose the next five, win the third five, and lose the last five; and about twenty more would lose the first five, win the next, lose the third five, and win the last five: about forty players, therefore, who seemed bound to win and lose always five games, and no more, in succession.

Again, if anyone had made a prediction that among the players of the twenty games there would be one who would win the first, then lose two, then win three, then lose four, then win five, and then lose the remaining five—and yet a sixth if the twenty-first game were played—that prophet would certainly be justified by the result. For about a score would be sure to have just such fortunes as he had indicated up to the twentieth game, and of these, nine or ten would be (practically) sure to win the twenty-first game also.

We see, then, that all the different kinds of luck—good, bad, indifferent, or changing—which believers in luck recognise, are bound to appear when any considerable number of trials are made; and all the varied ideas which men have formed respecting fortune and her ways are bound to be confirmed.

It may be asked by some whether this is not proving that there is such a thing as luck instead of overthrowing the idea of luck. But such a question can

only arise from a confusion of ideas as to what is meant by luck. If it be merely asserted that such and such men have been lucky or unlucky, no one need dispute the proposition; for among the millions of millions of millions of purely fortuitous events affecting the millions of persons now living, it could not but chance that the most remarkable combinations, sequences, alternations, and so forth, of events, lucky or unlucky, must have presented themselves in the careers of hundreds. Our illustrative case, artificial though it may seem, is in reality not merely an illustration of life and its chances, but may be regarded as legitimately demonstrating what must inevitably happen on the wider arena and amid the infinitely multiplied vicissitudes of life. But the belief in luck involves much more. The idea involved in it, if not openly expressed (usually expressed very freely), is that some men are lucky by nature, others unlucky, that such and such times and seasons are lucky or unlucky, that the progress of events may be modified by the lucky or unlucky influence of actions in no way relating to them; as, for instance, that success or failure at cards may be affected by the choice of a seat, or by turning round thrice in the seat. This form of belief in luck is not only akin to superstition, it *is* superstition. Like all superstition, it is mischievous. It is, indeed, the very essence of the gambling spirit, a spirit so demoralising that it blinds men to the innate immorality of gambling. It is this belief in luck, as something which can be relied on, or propitiated, or influenced by such and such practices,

which is shown, by reasoning and experience alike, to be entirely inconsistent not only with facts but with possibility.

But oddly enough, the believers in luck show by the form which their belief takes that in reality they have no faith in luck any more than men really have faith in superstitions which yet they allow to influence their conduct. A superstition is an idle dread, or an equally idle hope, not a real faith; and in like manner is it with luck. A man will tell you that at cards, for instance, he always has such and such luck; but if you say, 'Let us have a few games to see whether you will have your usual luck,' you will usually find him unwilling to let you apply the test. If you try it, and the result is unfavourable, he argues that such peculiarities of luck never do show themselves when submitted to test. On the other hand, if it so chances that on that particular occasion he has the kind of luck which he claims to have *always*, he expects you to accept the evidence as decisive. Yet the result means in reality only that certain events, the chances for and against which were probably pretty equally divided, have taken place.

So, if a gambler has the notion (which seems to the student of science to imply something little short of imbecility of mind) that turning round thrice in his chair will change the luck, he is by no means corrected of the superstition by finding the process fail on any particular occasion. But if the bad luck which has hitherto pursued him chances (which it is quite as

likely to do as not) to be replaced by good or even by moderate luck, after the gambler has gone through the mystic process described, or some other equally absurd and irrelevant manœuvre, then the superstition is confirmed. Yet all the time there is no real faith in it. Such practices are like the absurd invocation of Indian 'medicine men'; there is a sort of vague hope that something good may come of them, no real faith in their efficacy.

The best proof of the utter absence of real faith in superstitions about luck, even among gambling men, the most superstitious of mankind, may be found in the incongruity of their two leading ideas. If there are two forms of expression more frequently than any others in the mouth of gambling men, they are those which relate to being in luck or out of luck on the one hand, and to the idea that luck must change on the other. Professional gamblers, like Steinmetz and his kind, have become so satisfied that these ideas are sound, whatever else may be unsound, in regard to luck, that they have invented technical expressions to present these theories of theirs, failing utterly to notice that the ideas are inconsistent with each other, and cannot both be right—though both may be wrong, and are so.

A player is said to be 'in the vein' when he has for some time been fortunate. He should only go on playing, if he is wise, at such a time, and at such a time only should he be backed. Having been lucky he is likely, according to this notion, to continue lucky.

But, on the other hand, the theory called 'the maturity of the chances' teaches that the luck cannot continue more than a certain time in one direction; when it has reached maturity in that direction it must change. Therefore, when a man has been 'in the vein' for a certain time (unfortunately no Steinmetz can say precisely how long), it is unsafe to back him, for he must be on the verge of a change of luck.

Of course the gambler is confirmed in his superstition, whichever event may befall in such cases. When he wins he applauds himself for following the luck, or for duly anticipating a change of luck, as the case may be; when he loses, he simply regrets his folly in not seeing that the luck must change, or in not standing by the winner.

And with regard to the idea that luck must change, and that in the long run events must run even, it is noteworthy how few gambling men recognise either, on the one hand, how inconsistent this idea is with their belief in luck which may be trusted (or, in their slang, may be safely backed), or, on the other hand, the real way in which luck 'comes even' after a sufficiently long run.

A man who has played long with success goes on because he regards himself as lucky. A man who has played long without success goes on because he considers that the luck is bound to change. The latter goes on with the idea that, if he only plays long enough, he must at least at some time or other recover his losses.

Now there can be no manner of doubt that if a man,

possessed of sufficient means, goes on playing for a very long time, his gains and losses will eventually be very nearly equal; assuming always, of course, that he is not swindled—which, as we are dealing with gambling men, is perhaps a sufficiently bold assumption. Yet it by no means follows that, if he starts with considerable losses, he will ever recover the sum he has thus had to part with, or that his losses may not be considerably increased. This sounds like a paradox; but in reality the real paradox lies in the opposite view.

This may be readily shown.

The idea to be controverted is this: that if a gambler plays long enough there must come a time when his gains and his losses are exactly balanced. Of course, if this were true, it would be a very strong argument against gambling; for what but loss of time can be the result of following a course which must inevitably lead you, if you go on long enough, to the place from which you started? But it is not true. If it were true, of course it involves the inference that, no matter when you enter on a course of gambling, you are bound after a certain time to find yourself where you were at *that* beginning. It follows that if (which is certainly possible) you lose considerably in the first few weeks or months of your gambling career, then, if you only play long enough you must inevitably find yourself as great a loser, on the whole, as you were when you were thus in arrears through gambling losses; for your play may be quite as properly considered to have begun when those losses had just been incurred, as to

have begun at any other time. Hence this idea that, in the long run, the luck must run even, involves the conclusion that, if you are a loser or a gainer in the beginning of your play, you must at some time or other be equally a gainer or loser. This is manifestly inconsistent with the idea that long-continued play will inevitably leave you neither a loser nor a gainer. If, starting from a certain point when you are a thousand pounds in arrears, you are certain some time or other, if you only play long enough, to have gained back that thousand pounds, it is obvious that you are equally certain some time or other (from that same starting-point) to be yet another thousand pounds in arrears. For there is no line of argument to prove you must regain it, which will not equally prove that some time or other you must be a loser by that same amount, over and above what you had already lost when beginning the games which were to put you right. If, then, you are to come straight, you must be able certainly to recover two thousand pounds, and by parity of reasoning four thousand, and again twice that; and so on *ad infinitum*: which is manifestly absurd.

The real fact is, that while the laws of probabilities do undoubtedly assure the gambler that his losses and gains will in the long run be nearly equal, the kind of equality thus approached is not an equality of actual amount, but of proportion. If two men keep on tossing for sovereigns, it becomes more and more unlikely, the longer they toss, that the difference between them will fall short of any given sum. If they go on till

they have tossed twenty million times, the odds are heavily in favour of one or the other being a loser of at least a thousand pounds. But the proportion of the amount won by one altogether, to the amount won altogether by the other, is almost certain to be very nearly a proportion of equality. Suppose, for example, that at the end of twenty millions of tossings, one player is a winner of 1,000*l.*, then he must have won in all 10,000,500*l.*, the other having won in all 9,999,500*l.*; the ratio of these amounts is that of 100005 to 99995, or 20001 to 19999. This is very nearly the ratio of 10000 to 9999, or is scarcely distinguishable, practically, from actual equality. Now if these men had only tossed eight times for sovereigns, it might very well have happened that one would have won five or six times, while the other had only won thrice or twice. Yet with a ratio of 5 to 3, or 3 to 1, against the loser, he would actually be out of pocket only 2*l.* in one case and 4*l.* in the other; while in the other case, with a ratio of almost perfect equality, he would be the loser of a thousand pounds.

But now it might appear that, after all, this is proving too much, or, at any rate, proves as much on one side as on the other; for if one player loses the other must gain; if a certain set of players lose the rest gain: and it might seem as though, with the prevalent ideas of many respecting gambling games, the chance of winning were a sufficient compensation for the chance of losing.

Where a man is so foolish that the chance of having

more money than he wants is equivalent in his mind (or what serves him for a mind) to the risk of being deprived of the power of getting what is necessary for himself and for his family, such reasoning may be regarded as convincing. For those who weigh their wants and wishes rightly, it has no value whatever. On the contrary it may be shown that every wager or gambling transaction, by a man of moderate means, definitely reduces the actual value of his possessions, even if the wager or transaction be a fair one. If a man who has a hundred pounds available to meet his present wants wagers 50*l.* against 50*l.*, or an equal chance, he is no longer worth 100*l.* He *may*, when the bet is decided, be worth 150*l.*, or he may be worth only 50*l.* All he can *estimate* his property at is about 87*l.* Supposing the other man to be in the same position, they are both impoverished as soon as they have made the bet; and when the wager is decided, the average value of their possessions in ready money is less than it was; for the winner gains less by having his 100*l.* raised to 150*l.* (or increased as 2 to 3), than the loser suffers by having his ready money halved.

Similar remarks apply to participation in lottery schemes, or the various forms of gambling at places like San Carlo. Every sum wagered means, at the moment when it is staked, a depreciation of the gambler's property; and would mean that, even if the terms on which the wagering were conducted were strictly fair. But this is never the case. In all lotteries and in all established systems of gambling certain odds are always

retained in favour of those who work the lottery or the gambling system. These odds make gambling in either form still more injurious to those who take part in it. Winners of course there are, and in some few cases winners may retain a large part of their gains, or at any rate expend them otherwise than in fresh gambling. Yet it is manifest that, apart from the circumstance that the *effects* of the gambling gains of one set of persons never counterbalance the *effects* of the gambling losses of others, there is always a large deduction to be made on account of the wild and reckless waste of money won by gambling. In many cases, indeed, large gambling gains have brought ruin to the unfortunate winner: set 'on horseback' by lightly acquired wealth, and unaccustomed to the position, he has ridden 'straightway to the devil.'

But the greed for chance-won wealth is so great among men of weak minds, and they are so large a majority of all communities, that the bait may be dangled for them without care to conceal the hook. In all lotteries and gambling systems which have yet been known the hook has been patent, and the evil it must do if swallowed should have been obvious. Yet it has been swallowed greedily.

A most remarkable illustration of the folly of those who trust in luck, and the cool audacity of those who trust in such folly, with more reason but with more rascality, is presented by the Louisiana Lottery in America. This is the only lottery of the kind now permitted in America. Indeed, it is nominally restricted

to the State of Louisiana; but practically the whole country takes part in it, tickets being obtainable by residents in every State of the Union. The peculiarity of the lottery is *the calm admission, in all advertisements, that it is a gross and unmitigated swindle.* The advertisements announce that each month 100,000 tickets will be sold, each at five dollars, shares of one-fifth being purchasable at one dollar. Two commissioners—Generals Early and Beauregard—control the drawings; so that we are told, and may well believe, the drawings are conducted with fairness and honesty, and in good faith to all parties. So far all is well. We see that each month, if all the tickets are sold, the sum of 500,000 dols. will be paid in. From this monthly payment we must deduct 1,000 dols. paid to each of the commissioners, and perhaps some 3,000 dols. at the outside for advertising. We may add another sum of 5,000 dols. for incidental expenses, machinery, sums paid to agents as commission on the sale of tickets, and so forth. This leaves 490,000 dols. monthly if all the tickets are sold. And as the lottery is 'incorporated by the State Legislature of Louisiana for charitable and educational purposes,' we may suppose that a certain portion of the sum paid in monthly will be set aside to represent the proceeds of the concern, and justify the use of so degrading a method of obtaining money. Probably it might be supposed that 24 per cent. per annum, or 2 per cent. per month, would be a fair return in this way, the system being entirely free from risk. This would amount to 9,800 dols., or say 10,000 dols.,

monthly. Those who manage the lottery are not content, however, with any such sum as this, which would leave 480,000 dols. to be distributed in prizes. They distribute 215,000 dols. less, the total amount given in prizes amounting to only 265,000 dols. If the 100,000 tickets are all sold—and it is said that few are ever left—the monthly profit on the transaction is not less than 225,000 dols., or 45 per cent. on the total amount received per month. This would correspond to 540 per cent. per annum if it were paid on a capital of 500,000 dols. But in reality it amounts to much more, as the lottery company runs no risk whatsoever. The Louisiana Lottery is a gross swindle, besides being disreputable in the sense in which all lotteries are so. What would be thought if a man held an open lottery, to which each of one hundred persons admitted paid 5*l.*, and taking the sum of 500*l.* thus collected, were to say: 'The lottery, gentlemen gamblers, will now proceed; 265*l.* of the sum before me I will distribute in prizes, as follows' (indicating the number of prizes and their several amounts); 'the rest, this sum of 235*l.*, which I have here separated, I will put into my own pocket' (suiting the action to the word) 'for my trouble in getting up this lottery'? The Louisiana Lottery is a transaction of the same rascally type—not rendered more respectable by being on a very much larger scale.

If the spirit of rash speculation will let men submit to swindling so gross as this, we can scarcely see any limit to its operation. Yet hundreds of thousands yield to the temptation thus offered, to gain suddenly a large

sum, at the expense of a small sum almost certainly lost, and partly stolen.

It should be known—though, perhaps, even this knowledge would not keep the moths away from the destruction to which they seem irresistibly lured—that gambling carried on long enough is not probable but certain ruin. There is no sum, however large, which is not certain to be absorbed at some time in the continuance of a sufficiently long series of trials, even at fair risks. Gamblers with moderate fortunes overlook this. In their idea, mistaken as it is, that luck must run even at last, they forget that, before that last to which they look has been reached, their last shilling may have gone. If they were content even to stay till—possibly—gain balanced loss, there would be some chance of escape. But what real gambler ever was content with such an aim as that? Luck must not only turn till loss has been recouped, but run on till great gains have been made. And no gambler was ever yet content to stay his hand when winning, or to give up when he began to lose again. The fatal faith in eventual good luck is the source of all bad luck; it is in itself the worst luck of all. Every gambler has this faith, and no gambler who holds to it is likely long to escape ruin.

GAMBLERS' FALLACIES.

It might be supposed that those who are most familiar with the actual results which present themselves in long series of chance games would form the most correct views respecting the conditions on which such results depend—would be, in fact, freest from all superstitious ideas respecting chance or luck. The gambler who sees every system—his own infallible system included—foiled by the run of events, who witnesses the discomfiture of one gamester after another that for a time had seemed irresistibly lucky, and who can number by hundreds those who have been ruined by the love of play, might be expected to recognise the futility of all attempts to anticipate the results of chance combinations. It is, however, but too well known that the reverse is the case. The more familiar a man becomes with the multitude of such combinations, the more confidently he believes in the possibility of foretelling—not, indeed, any special event, but—the general run of several approaching events. There has never been a successful gambler who has not believed that his success (temporary though such success ever is, where games of pure chance are concerned) has been the result of

skilful conduct on his own part; and there has never been a ruined gambler (though ruined gamblers are to be counted by thousands) who has not believed that when ruin overtook him he was on the very point of mastering the secret of success. It is this fatal confidence which gives to gambling its power of fascinating the lucky as well as the unlucky. The winner continues to tempt fortune, believing all the while that he is exerting some special aptitude for games of chance, until the inevitable change of luck arrives; and thereafter he continues to play because he believes that his luck has only deserted him for a time, and must presently return. The unlucky gambler, on the contrary, regards his losses as sacrifices to ensure the ultimate success of his 'system,' and even when he has lost his all, continues firm in the belief that had he had more money to sacrifice he could have bound fortune to his side for ever.

I propose to consider some of the most common gambling superstitions—noting, at the same time, that like superstitions prevail respecting chance events (or what is called fortune) even among those who never gamble.

Houdin, in his interesting book, *Les Tricheries des Grecs dévoilées*, has given some amusing instances of the fruits of long gaming experience. 'They are presented,' says Steinmetz, from whose work, *The Gaming Table*, I quote them, 'as the axioms of a professional gambler and cheat.' Thus we might expect that, however unsatisfactory to men of honest mind, they

would at least savour of a certain sort of wisdom. Yet these axioms, the fruit of long study directed by self-interest, are all utterly untrustworthy.

'Every game of chance,' says this authority, 'presents two kinds of chances that are very distinct—namely, those relating to the person interested, that is the player; and those inherent in the combinations of the game.' That is, we are to distinguish between the chances proper to the game, and those depending on the luck of the player. Proceeding to consider the chances proper to the game itself, our friendly cheat sums them all up in two rules. First:—'Though chance can bring into the game all possible combinations, there are nevertheless certain limits at which it seems to stop: such, for instance, as a certain number turning up ten times in succession at roulette; this is possible, but it has never happened.' Secondly:—'In a game of chance, the oftener the same combination has occurred in succession, the nearer we are to the certainty that it will not recur at the next cast or turn up. This is the most elementary of the theories on probabilities; it is termed *the maturity of the chances*' (and he might have added that the belief in this elementary theory had ruined thousands). 'Hence,' he proceeds, 'a player must come to the table not only "in luck," but he must not risk his money except at the instant prescribed by the rules of the maturity of the chances.' Then follow the precepts for personal conduct:—'For gaming prefer roulette, because it presents several ways of staking your money—which permits the study of several. A

player should approach the gaming-table perfectly calm and cool—just as a merchant or tradesman in treaty about any affair. If he gets into a passion it is all over with prudence, all over with good luck—for the demon of bad luck invariably pursues a passionate player. Every man who finds a pleasure in playing runs the risk of losing.[1] A prudent player, before undertaking anything, should put himself to the test to discover if he is "in vein" or in luck. In all doubt he should abstain. There are several persons who are constantly pursued by bad luck: to such I say—*never play*. Stubbornness at play is ruin. Remember that Fortune does not like people to be overjoyed at her favours, and that she prepares bitter deceptions for the imprudent who are intoxicated by success. Lastly, before risking your money at play, study your "vein," and the different probabilities of the game—termed, as aforesaid, the maturity of the chances.'

Before proceeding to exhibit the fallacy of the principles here enunciated—principles which have worked incalculable mischief—it may be well to sketch the history of the scamp who enunciated them—so far, at least, as his gambling successes are concerned. His first meeting with Houdin took place at a subscription ball, where he managed to fleece Houdin 'and others to a considerable amount, contriving a dexterous

[1] This *naïve* admission would appear, as we shall presently see, to have been the fruit of genuine experience on our gambler's part: it only requires that, for the words 'runs the risk,' we should read 'incurs the certainty,' to be incontrovertible.

escape when detected. Houdin afterwards fell in with him at Spa, where he found the gambler in the greatest poverty, and lent him a small sum—to practise his grand theories.' This sum the gambler lost, and Houdin advised him 'to take up a less dangerous occupation.' It was on this occasion, it would seem, that the gambler revealed to Houdin the particulars recorded in his book. 'A year afterwards Houdin unexpectedly fell in with him again; but this time the fellow was transformed into what is called a "*demi-millionaire*," having succeeded to a large fortune on the death of his brother who died intestate. According to Houdin, the following was the man's declaration at the auspicious meeting: "I have," he said, "completely renounced gaming; I am rich enough; and care no longer for fortune. And yet," he added proudly "if I now cared for the thing, how I could break those bloated banks in their pride, and what a glorious vengeance I could take of bad luck and its inflexible agents! But my heart is too full of my happiness to allow the smallest place for the desire of vengeance."' Three years later he died; and Houdin informs us that he left the whole of his fortune to various charitable institutions, his career after his acquisition of wealth going far to demonstrate the justice of Becky Sharp's theory that it is easy to be honest on five thousand a year.

It is remarkable that the principles enunciated above are not merely erroneous, but self-contradictory. Yet it is to be noticed that though they are presented as

the outcome of a life of gambling experiences, they are in reality entertained by all gamblers, however limited their experience, as well as by many who are only prevented by the lack of opportunity from entering the dangerous path which has led so many to ruin. These contradictory superstitions may be called severally—the gambler's belief in his own good luck, and his faith in the turn of luck. When he is considering his own fortune he does not hesitate to believe that on the whole the Fates will favour him, though this belief implies in reality the *persistence* of favourable conditions. On the contrary, when he is considering the fortunes of others who are successful in their play against him, he does not doubt that their good luck will presently desert them, that is, he believes in the *non-persistence* of favourable conditions in their case.

Taking in their order the gambling superstitions which have been presented above, we have, first of all, to inquire what truth there is in the idea that there are limits beyond which pure chance has no power of introducing peculiar combinations. Let us consider this hypothesis in the light of actual experience. Mr. Steinmetz tells us that, in 1813, a Mr. Ogden wagered 1,000 guineas to one that 'seven' would not be thrown with a pair of dice ten successive times. The wager was accepted (though it was egregiously unfair), and strange to say his opponent threw 'seven' *nine times running*. At this point Mr. Ogden offered 470 guineas to be off the bet. But his opponent declined (though the price offered was far beyond the real value of his

chance). He cast yet once more, and threw 'nine,' so that Mr. Ogden won his guinea.

Now here we have an instance of a most remarkable series of throws, the like of which has never been recorded before or since. Before those throws had been made, it might have been asserted that the throwing of nine successive 'sevens' with a pair of dice was a circumstance which chance could never bring about, for experience was as much against such an event as it would seem to be against the turning up of a certain number ten successive times at *roulette*. Yet experience now shows that the thing is possible; and if we are to limit the action of chance, we must assert that the throwing of 'seven' *ten* times in succession is an event which will never happen. Yet such a conclusion obviously rests on as unstable a basis as the former, of which experience has disposed. Observe, however, how the two gamblers viewed this very eventuality. Nine successive 'sevens' had been thrown; and if there were any truth in the theory that the power of chance was limited, it might have been regarded as all but certain that the next throw would not be a 'seven.' But a run of bad fortune had so shaken Mr. Ogden's faith in his luck (as well as in the theory of the 'maturity of the chances') that he was ready to pay 470 guineas (nearly thrice the mathematical value of his opponent's chance) in order to save his endangered thousand; and so confident was his opponent that the run of luck would continue that he declined this very favourable offer. Experience had

in fact shown both the players, that although 'sevens' could not be thrown for ever, yet there was no saying when the throw would change. Both reasoned probably that as an eighth throw had followed seven successive throws of 'seven' (a wonderful chance), and as a ninth had followed eight successive throws (an unprecedented event), a tenth might well follow the nine (though hitherto no such series of throws had ever been heard of). They were forced as it were by the run of events to reason justly as to the possibility of a tenth throw of 'seven'—nay, to exaggerate that possibility into probability; and it appears from the narrative that the strange series of throws quite checked the betting propensities of the bystanders, and that not one was led to lay the wager (which according to ordinary gambling superstitions would have been a safe one) that the tenth throw would not give 'seven.'

We have spoken of the unfairness of the original wager. It may interest our readers to know exactly how much should have been wagered against a single guinea, that ten 'sevens' would not be thrown. With a pair of dice there are thirty-six possible throws, and six of these give 'seven' as the total. Thus the chance of throwing 'seven' is one sixth, and the chance of throwing 'seven' ten times running is obtained by multiplying six into itself ten times, and placing the resulting number under unity, to represent the minute fractional chance required. It will be found that the number thus obtained is 60,466,176, and instead of 1,000 guineas, fairness required that 60,466,175

guineas should have been wagered against one guinea, so enormous are the chances against the occurrence of ten successive throws of 'seven.' Even against nine successive throws the fair odds would have been 10,077,595 to one, or about forty thousand guineas to a farthing. But when the nine throws of 'seven' had been made, the chance of a tenth throw of 'seven' was simply one-sixth as at the first trial. If there were any truth in the theory of the 'maturity of the chances,' the chance of such a throw would of course be greatly diminished. But even taking the mathematical value of the chance, Mr. Ogden need in fairness only have offered a sixth part of 1,001 guineas (the amount of the stakes), or 166 guineas 17*s.* 6*d.*, to be off his wager. So that his opponent accepted in the first instance an utterly unfair offer, and refused in the second instance a sum exceeding by more than three hundred guineas the real value of his chance.

Closely connected with the theory about the range of possibility in the matter of chance combinations, is the theory of the maturity of the chances—'the most elementary of the theories on probabilities.' It might safely be termed the most mischievous of gambling superstitions.

As an illustration of the application of this theory, we may cite the case of an Englishman, once well known at foreign gambling-tables, who had based a system on a generalisation of this theory. In point of fact the theory asserts that when there has been a run in favour of any particular event, the chances in favour

of the event are reduced, and therefore, necessarily, the chances in favour of other events are increased. Now our Englishman watched the play at the *roulette* table for two full hours, carefully noting the numbers which came up during that time. Then, eschewing those numbers which had come up oftenest, he staked his money on those which had come up very seldom or not at all. Here was an infallible system according to 'the most elementary of the theories of probability.' The tendency of chance-results to right themselves, so that events equally likely in the first instance will occur an equal number of times in the long run, was called into action to enrich our gambler and to ruin the unlucky bankers. Be it noted, in passing, that events do thus right themselves, though this circumstance does not operate quite as the gambler supposed, and cannot be trusted to put a penny into any one's pocket. The system was tried, however, and instead of reasoning respecting its soundness, we may content ourselves with recording the result. On the first day our Englishman won more than seven hundred pounds in a single hour. 'His exultation was boundless. He thought he had really discovered the "philosopher's stone." Off he went to his bankers, and transmitted the greater portion of his winnings to London. The next day he played and lost fifty pounds; and the following day he achieved the same result, and had to write to town for remittances. In fine, in a week he had lost all the money he won at first, with the exception of fifty pounds, which he reserved to take

him home; and being thoroughly convinced of the exceeding fickleness of fortune, he has never staked a sixpence since, and does all in his power to dissuade others from playing.'[1]

He took a very sound principle of probabilities as the supposed basis of his system, though in reality he entirely mistook the nature of the principle. That principle is, that where the chances for one or another of two results are equal for each trial, and many trials are made, the number of events of one kind will bear to those of the other kind a very nearly equal ratio: the greater the number of events, the more nearly will the ratio tend to equality. This is perfectly true; and nothing could be safer than to wager on this principle. Let a man toss a coin for an hour, and I would wager confidently that neither will 'heads' exceed 'tails,' or 'tails' exceed 'heads' in a greater ratio than that of 21 to 20. Let him toss for a day, and I would wager as confidently that the inequality will not be greater than that represented by the ratio of 101 to 100. Let the tossing be repeated day after day for a year, and I would wager my life that the disproportion will be less than that represented by the ratio of 1,001 to 1,000. Yet so little does this principle bear the interpretation placed upon it by the inventor of the system above described, that if on any occasion during this long-continued process of tossings 'head had been tossed (as it certainly would often be) no less than twenty times in

[1] From an interesting paper entitled 'Le Jeu est fait,' in *Chambers's Journal.*

succession, I would not wager a sixpence on the next tossing giving 'tail,' or trust a sixpence to the chance of 'tail' appearing oftener than 'head' in the next five, ten, or twenty tossings. Not only should reason show the utter absurdity of supposing that a tossing, or a set of five, ten, or twenty tossings, can be affected one way or the other by past tossings, whether proximate or remote; but the experiment has been tried, and it has appeared (as might have been known beforehand) that after any number of cases in which 'heads' (say) have appeared such and such a number of times in succession, the next tossing has given 'heads' as often as it has given 'tails.' Thus, in 124 cases, Buffon, in his famous tossing trial, tossed 'tails' four times running. On the next trial, in these 124 cases, 'head' came 56 times and 'tail' 68 times. So most certainly the tossing of 'tail' four times running had not diminished the tendency towards 'tail' being tossed. Among the 68 cases which had thus given 'tail' five times running, 29 failed to give another 'tail,' while the remaining 39 gave another, that is, a sixth 'tail.' Of these 39, 25 failed to give another 'tail,' while 14 gave a seventh 'tail'; and here it might seem we have evidence of the effect of preceding tosses. The disproportion is considerable, and even to the mathematician the case is certainly curious; but in so many trials such curiosities may always be noticed. That it will not bear the interpretation put upon it is shown by the next steps. Of the 14 cases, 8 failed to give another 'tail,' while the remaining six gave another, that is, an eighth 'tail'; and these

numbers eight and six are more nearly equal than the preceding numbers 25 and 14; so that the tendency to change had certainly not increased at this step. However, the numbers are too small in this part of the experiment to give results which can be relied upon. The cases in which the numbers were large prove unmistakably, what reason ought to have made self-evident, that past events of pure chance cannot in the slightest degree affect the result of sequent trials.

To suppose otherwise is, indeed, utterly to ignore the relation between cause and effect. When anyone asserts that because such and such things have happened, therefore such and such other events will happen, he ought at least to be able to show that the past events have some direct influence on those which are thus said to be affected by them. But if I am going to toss a coin perfectly at random, in what possible way can the result of the experiment be affected by the circumstance that during ten or twelve minutes before, I tossed 'head' only or 'tail' only?

The system of which I now propose to speak is more plausible, less readily put to the full test, and consequently far more dangerous than the one just described. In it, as in the other, reliance is placed on a 'change' after a 'run' of any kind, but not in the same way.

Everyone is familiar with the method of renewing wagers on the terms 'double' or 'quits.' It is a very convenient way of getting rid of money which has been won on a wager by one who does not care for wagering, and, not being to the manner born, does not feel com-

fortable in pocketing money won in this way. You have rashly backed some favourite oarsman, let us say, or your college boat, or the like, for a level sovereign, not caring to win, but accepting a challenge to so wager rather than seem to want faith in your friend, college, or university. You thus find yourself suddenly the recipient of a coin to which you feel you are about as much entitled as though you had abstracted it from the other bettor's pocket. You offer him 'double or quits,' tossing the coin. Perhaps he loses, when you would be entitled to two sovereigns. You repeat the offer, and if he again loses (when you are entitled to four sovereigns), you again repeat it, until at last he wins the toss. Then you are 'quits,' and can be happy again.

The system of winning money corresponds to this safe system of getting rid of money which has been uncomfortably won. Observe that if you only go on long enough with the double-or-quits method, as above, you are sure to get rid of your sovereign; for your friend cannot go on losing for ever. He might, indeed, lose nine or ten times running, when he would owe you 512*l.* or 1,024*l.*; and if he then lost heart, while yet he regarded his loss, like his first wager, as a debt of honour from which you could not release him, matters would be rather awkward. If he lost twenty times he would owe you a million, which would be more awkward still; except that, having gone so far, he could not make matters worse by going a little farther; and in a few more tossings you would get rid of your millions as completely as of the sovereign first won. Still, speaking

generally, this double-or-quits method is a sure and easy way of clearing such scores. But it may be reversed and become a pretty sure and easy way of making money.

Suppose a man, whom we will call A, to wager with another, B, one sovereign on a tossing (say). If he wins, he gains a sovereign. Suppose, however, he loses his sovereign. Then let him make a new wager of two sovereigns. If he wins, he is the gainer of one sovereign in all: if he loses, he has lost three in all. In the latter case let him make a new wager, of four sovereigns. If he wins, he gains one sovereign; if he loses, he has lost seven in all. In this last case let him wager eight sovereigns. Then, if he wins, he has gained one sovereign, and if he loses he has lost fifteen. Wagering sixteen sovereigns in the latter case, he gains one in all if he wins, and has lost thirty-one in all if he loses. So he goes on (supposing him to lose each time) doubling his wager continually, until at last he wins. Then he has gained one sovereign. He can now repeat the process, gaining each time a sovereign whenever he wins a tossing. And manifestly in this way A can most surely and safely win every sovereign B has. Yet every wager has been a perfectly fair one. We seem, then, to see our way to a safe way of making any quantity of money. B, of course, would not allow this sort of wagering to go on very long. But the bankers of a gambling establishment undertake to accept any wagers which may be offered, on the system of their game, whether *rouge-et-noir*, roulette, or what not,

between certain limits of value in the stakes. Say these limits are from 5*s.* to 100*l.*, as I am told is not uncommonly the case. A man may wager 5*s.* on this plan, and double eight times before his doublings carry the stake above 100*l.* Or with more advantage he may let the successive stakes be such that the eighth doubling will make the maximum sum, or 100*l.*; so that the stakes in inverse order will be 100*l.*, 50*l.*, 25*l.*, 12*l.* 10*s.*, 6*l.* 5*s.*, 3*l.* 2*s.* 6*d.*, 1*l.* 11*s.* 3*d.*, 15*s.* 7*d.* (fractions of a penny not being allowed, I suppose[1]), and, lastly, 7*s.* 9*d.*; nine stakes, or eight doublings in all. It is so utterly unlikely, says the believer in this system, that where the chances are practically equal on two events, the same event will be repeated nine times running, that I may safely apply this method, gaining at each venture ('though really there is no risk at all') 7*s.* 9*d.*, until at last I shall accumulate in this way a small fortune, which in time will become a large fortune.

The proprietors of gambling houses naturally encourage this pleasing delusion. They call this power of varying the stakes a very important advantage possessed by the player at such tables. They say, truly enough, a single player would not wager if the stakes could be varied in this manner, and he possessed no power of refusing any offer between such limits. Since a single player would refuse to allow this arrangement, it is manifest the arrangement is a privilege. Being a

[1] Possibly pence are not allowed, in which case the successive stakes would be 7*s.*, 14*s.*, 1*l.* 8*s.*, 2*l.* 16*s.*, 5*l.* 12*s.*, 11*l.* 4*s.*, 22*l.* 8*s.*, 44*l.* 16*s.*, and lastly, 89*l.* 12*s.*

privilege, it is worth paying for. It is on this account that we poor bankers, who oblige those possessed of gambling propensities by allowing them to exercise their tastes that way, must have a certain small percentage of odds in our favour. Thus at *rouge-et-noir* we really must have one of the "refaits" allowed us, say the first, the *trente-et-un*, though any other would suit us equally well: but even then we do not win what is on the table; the *refait* may go against us, when the players save their stakes, and if we win we only win what has been staked on one colour,' and so forth.

Those who like gambling, too, and so like to believe that the bankers are strictly fair, adopt this argument. Thus the editor of *The Westminster Paper* says: 'The Table at all games has an extra chance, a chance varying from one zero at one table to two at another; that is a chance every player understands when he sits down to play, *and it is perfectly fair and honest* (! !) That this advantage over a long series must tell is as certain as that two and two make four. But . . . . the bank does not always win; on the contrary,' we often 'hear of the bank being broken and closed until more cash is forthcoming. The number of times the bank loses and nothing is said about it, would amount to a considerable number of times in the course of a year. A small percentage on one side or the other, extended over a long enough series, will tell; but on a single event the difference in the gambler's eyes' (yes, truly, in *his* eyes) 'is small. For that percentage the punter is enabled to vary his stakes from 5*s*. say, to 100*l*. With-

out some such advantage, no one would permit his adversaries thus to vary the stakes. The punter' (poor moth !) 'is willing to pay for this advantage.'

And all the while the truth is that the supposed advantage is no advantage at all—at least, to the player. It is of immense advantage to the bankers, because it encourages so many to play who otherwise might refrain. But in reality the bankers would make the same winnings if every stake were of a fixed amount, say 10*l.*, as when the stakes can be varied—always assuming that as many players would come to them, and play as freely, as on the present more attractive system.

Let us consider the actual state of the case, when a player at a table doubles his stakes till he wins—repeating the process from the lowest stakes after each success.

But first—or rather, as a part of this inquiry—let us consider why our imaginary player B would decline to allow A to double wagers in the manner described. In reality, of course, A's power of doubling is limited by the amount of A's money, or of his available money for gambling. He cannot go on doubling the stakes when he has paid away more than half his money. Suppose, for instance, he has 1,000*l.* in notes and 30*l.* or so in sovereigns. He can wager successively (if he loses so often) 1*l.*, 2*l.*, 4*l.*, 8*l.*, 16*l.*, 32*l.*, 64*l.*, 128*l.* 256*l.*, 512*l.*, or ten times. But if he loses his last wager he will have paid away 1,023*l.*, and must stop for the time, leaving B the gainer of that sum. This is a very unlikely result for a single trial. It would not be

likely to happen in a hundred or in two hundred trials, though it might happen at the first trial, or at a very early one. Even if it happened after five hundred trials, A would only have won 500*l.* in those, and B winning 1,023*l.* at the last, would have much the better of the encounter.

Why, then, would not B be willing to wager on these terms? For precisely the same reason (if he actually reasoned the matter out) that he should be unwilling to pay 1*l.* for one ticket out of 1,024 where the prize was 1,024*l.* Each ticket would be fairly worth that sum. And many foolish persons, as we know, are willing to pay in that way for a ticket in a lottery, even paying more than the correct value. But no one of any sense would throw away a sovereign for the chance (even truly valued at a sovereign) of winning a thousand pounds. That, really, is what B declines to do. Every venture he makes with A (supposing A to have about 1,000*l.* at starting, and so to be able to keep on doubling up to 512*l.*) is a wager on just such terms. B wins nothing unless he wins 1,024*l.*; he loses at each failure 1*l.* His chance of winning, too, is the same, at each venture, as that of drawing a single marked ticket from a bag containing 1,024 tickets. Each venture, though it may be decided at the first or second tossing, is a venture of ten tossings. Now, with ten tossings there are 1,024 possible results, any one of which is as likely as any other. One of these, and one only, is favourable to B, viz. the case of ten 'heads,' if he is backing 'heads,' or ten 'tails,' if he is backing 'tails.'

Thus he pays, in effect, one pound for one chance in 1,024 of winning 1,024*l.*, though, in reality, he does not pay the pound until the venture is decided against him; so that, if he wins, he receives 1,023*l.*, corresponding (with the 1*l.*) to the total just named.

Now, to wager a pound in this way, for the chance of winning 1,024*l.*, would be very foolish; and though continually repeating the experiment would in the long run make the number of successes bear the right proportion to the number of failures, yet B might be ruined long before this happened, though quite as probably A would be ruined. B's ruin, if effected, would be brought about by steadily continued small losses, A's by a casual but overwhelming loss. The richer B and A were, the longer it would be before one or other was ruined, though the eventual ruin of one or other would be certain. If one was much richer than the other, his chance of escaping ruin would be so much the greater, and so much greater, therefore, the risk of the poorer. In other words, the odds would be great in favour of the richer of the two, whether A or B, absorbing the whole property of the other, if wagering on this plan were continued steadily for a long time.

Now, if we extend such considerations as these to the case in which an individual player contends against a bank, we shall see that, even without any percentage on the chances, the odds would be largely in favour of the bank. If the player is persistent in applying his system, he is practically certain to be ruined. For it is to be noticed that in such a system the player is exposed

to that which he can least afford, namely, sudden and great loss; it is by such losses that his ruin will be brought about if at all. On the other hand, the bank, which can best afford such losses, has to meet only a steady slow drain upon its resources, until the inevitable *coup* comes which restore all that had been thus drained out, and more along with it. If the player were even to carry on his system in the manner which my reasoning has really implied; if, as he made his small gain at each venture, he set it by to form a reserve fund—even then his ruin would be inevitable in the long run. But every one knows that gamblers do nothing of the sort. 'Lightly come, lightly go,' is their rule, so far as their gains are concerned. [In another sense, their rule is, lightly come (to the gaming-table) and heavily go when the last pound has been staked and lost.] Thus they run a risk which, in their way of playing, amounts almost to a certainty of ruining themselves, and they do not even take the precaution which would alone give them their one small, almost evanescent chance of escape. On the other hand, the bankers, who are really playing an almost perfectly safe game, leave nothing to chance. The bulk of the money gained by them is reserved to maintain the balance necessary for safety. Only the actual profits of their system—the percentage of gain due to their percentage on the chances—is dealt with as income; that is, as money to be spent.

It is true that in one sense the case between the bankers and the public resembles that of a player with a small capital against a player with a large capital;

the bankers have indeed a large capital, but it is small compared with that of the public at large who frequent the gaming-tables. But, in the first place, this does not at all help any single player. It is all but certain that the public (meaning always the special gaming public) will not be ruined as a whole, just as it is all but certain that the whole of an army engaged in a campaign, even under the most unfavourable circumstances, will not be destroyed if recruits are always available at short notice. Now, if the circumstances of a campaign are such that each individual soldier runs exceeding risk of being killed, it will not improve the chances of any single soldier that the army as a whole will not be destroyed; and in like manner those who gamble persistently are not helped in their ruin by the circumstance that, as one is 'pushed from the board, others ever succeed.' Even the chance of the bank being ruined, however, is not favourable to the gambler who follows such a system as I am dealing with, but positively adds to his risks. In the illustrative case of A playing B, the ruin of B meant that A had gained all B's money. But in the case of a gambler playing on the doubling system at a gaming-table, the ruin of the bank would be one of the chances against him that such a gambler would have to take into account. It might happen when he was far on in a long process of doubling, and would be almost certain to happen when he had to some degree entered on such a process. He would then be certainly a loser on that particular venture. If a winner on the event actually decided when the bank

broke (only one, be it remembered, of the series forming his venture), he would perhaps receive a share, but a share only, of the available assets. The rules of the table may be such that these will always cover the stakes, and in that case the player, supposing he had won on the last event decided, would sustain no loss. Should he have lost on that event, however, which ordinarily would at least not interfere with the operation of his system, he is prevented from pursuing the system till he has recouped his loss. This can never happen in play between two gamblers on this system. For the very circumstance that A has lost an event involves of necessity the possession by B of enough money to continue the system. B's stake after winning is always double the last stake, but after winning the amount just staked of course he must possess double that amount—since he has his winnings and also a sum at least equal, which he must have had when he wagered an equal stake. But when a player at the gaming-tables loses an event in one of his ventures, it by no means follows equally that the bank can continue to double (assuming the highest value allowed to have not been reached). Losses against other players may compel the bank to close when the system player has just lost a tolerably heavy *coup*. His system then is defeated, and he sustains a loss distinct in character from those which his system normally involves. In other words, the chances against him are increased; and, on the other hand, the bankers' chance of ruin would be small, even if they had no advantage in the odds, simply because the sum staked

bears a much smaller proportion to their capital than the wagers of the individual player bear to his property.

Yet the reader must not fall into the mistake of supposing that because the individual player would have enormous risks against him, even if the bankers took no percentage on the chances, the bank would then in the long run make enormous gains. That would be a paradoxical result; though at first sight it seems equally paradoxical to say that while every single player would be almost certain to be ruined the bank would not gain in the long run. This, however, is perfectly true. The fact is, that, among the few who escaped ruin, some would be enormous gainers. It would be because of some marvellous runs of luck, and consequent enormous gains, that they would be saved from ruin; and the chances would be that some among these would be very heavy gainers. They would be few; and the action of a man who gambled heavily on the chance of being one of these few, would be like that of a man who bought half a dozen tickets, at a price of 1,000*l.* each (his whole property being thus expended), among millions of tickets in a lottery, in which were a few prizes of 1,000,000*l.* each. But though the smallness of the chance of being one among the few very great gainers at the gambling-table, makes it absurd for a man to run the enormous risk of ruin involved in persistent play, yet, so far as the bankers would be concerned, the great losses on the few winners would in the long run equalise the moderate gains on the great majority of their customers. They would neither gain nor lose a sum

bearing any considerable proportion to their ventures, and would run some risk, though only a small one, of being swamped by a long-continued run of bad luck.

But the bankers do not in this way leave matters to chance. They take a percentage on the chances. The percentage they take is often not very large in itself, though it is nearly always larger than it appears, even when regarded properly as a percentage on the chances. But what is usually overlooked by those who deal with this matter, and especially by those who, being gamblers themselves, *want* to think that gaming houses give them very fair chances, is that a very small percentage on the chances may mean, and necessarily does mean, an enormous percentage of profits.

Let us take, as illustrating both the seeming smallness of the percentage on the chances, and the enormous probable percentage of profits, the game of *rouge-et-noir*, so far as it can be understood from the accounts given in the books.[1] I follow De Morgan's rendering

[1] De Morgan remarks on the incomplete and unintelligible way in which this game is described in the later editions of Hoyle. It is singular how seldom a complete and clear account of any game can be found in books, though written by the best card-players. I have never yet seen a description of cribbage, for example, from which anyone who knew nothing of the game, and could find no one to explain it practically to him, could form a correct idea of its nature. In half a dozen lines from the beginning of a description, technical terms are used which have not been explained, remarks are made which imply a knowledge on the reader's part of the general object of the game of which he should be supposed to know nothing, and many matters absolutely essential to a right apprehension of the nature of the game are not touched on from beginning

of these confused and imperfect accounts. It seems to be correct, for his computation of the odds for and against the player leads to the same result as Poisson obtained, who knew the game, though he nowhere gives a description of it.

A number of packs is taken (six, Hoyle says), 'and the cards are well mixed. Each common card counts for the number of spots on it, and the court cards are each reckoned as ten. A table is divided into two compartments, one called *rouge*, the other *noir*, and a player stakes his money in which he pleases. The proprietor of the bank, who risks against all comers, then lays down cards in one compartment until the number of spots exceeds thirty; as soon as this has happened, he proceeds in the same way with the other compartment.' The number of spots in each compartment is thus ne-

to end, or are so insufficiently described that they might as well have been left altogether unnoticed. It is the same with verbal descriptions. Not one person in a hundred can explain a game of cards respectably, and not one in a thousand can explain a game well A beginner can pick up a game after awhile, by combining with the imperfect explanations given him the practical illustrations which the cards themselves afford. But there is no reason in the nature of things why a written or a verbal description of such a game as whist or cribbage should not suffice to make an attentive reader or hearer perfectly understand the nature of the game. From what I have noticed in this matter, I would assert with some confidence that anyone who can explain clearly, yet succinctly, a game at cards, must have the explanatory gift so exceptionally developed that he could most usefully employ it in the explanation of such scientific subjects as he might himself be able to master. I believe, too, that the student of science who desires to explain his subject to the general public, can find no better exercise, and few better tests, than the explanation of some simple game—the explanation to be sufficient for persons knowing nothing of the game.

cessarily between 31 and 40, both inclusive. The compartment in which the total number of spots is least is the winning one. Thus, if there are 35 spots on the cards in the *rouge*, and 32 on the cards in the *noir*, *noir* wins, and all players who staked upon *noir* receive from the bank sums equal to their stakes. The process is then repeated. So far, it will be observed, the chances are equal for the players and for the bankers. It will also be observed that the arrangement is one which strongly favours the idea (always encouraged by the proprietors of gaming houses) that the bankers have little interest in the result. For the bank does not back either colour. The players have all the backing to themselves. If they choose to stake more in all on the red than on the black, it becomes the bank's interest that black should win; but it was by the players' own acts that black became for the time the bank's colour. And not only does this suggest to the players the incorrect idea, that the bank has little real interest in the game, but it encourages the correct idea, which it is the manifest interest of the bankers to put very clearly before the players, that everything is fairly managed. If the bank chose a colour, some might think that the cards, however seemingly shuffled, were in reality arranged, or else were so manipulated as to make the bank's colour win oftener than it should do. But since the players themselves settle which shall be the bank's colour at each trial, there cannot be suspicion of foul play of this sort.

We now come to the bank's advantage on the

chances. The number of spots in the black and red compartments may be equal. In this case (called by Hoyle a *refait*) the game is drawn; and the players may either withdraw, increase, or diminish their stakes, as they please, for a new game, if the number of spots in each compartment is any except 31. But if the number in each be 31 (a case called by Hoyle a *refait trente-et-un*), then the players are not allowed to withdraw their stakes. And not only must the stakes remain for a new game, but, whatever happens on this new trial, the players will receive nothing. Their stakes are for the moment impounded (or technically, according to Hoyle, *en prison*). The new game (called an *après*), unless it chances to give another *refait*, will end in favour of either *rouge* or *noir*. Whichever compartment wins, the players in that compartment save their stakes, but receive nothing from the bank; the players who have put their stakes in the other compartment lose them. De Morgan says here, not quite correctly, 'should the bank win it takes the stakes, should the bank lose the player recovers his stakes.' This is incorrect, because it at least suggests the incorrect idea that the bank may either win or the stakes go clear; whereas in reality, except in the improbable event of all the players backing one colour, the bank is sure to win something, viz., either the stakes in the red or those in the black compartment, and the only point to be settled is whether the larger or the smaller of these probably unequal sums shall pass to the bank's exchequer. If the *après* gives a second *refait*, the stakes still remain impounded,

and another game is played, and no stakes are released until either *rouge* or *noir* has won. But in the meantime new stakes may be put down, before the fate of the impounded stakes has been decided.

Thus, whereas, with regard to games decided at the first trial, the bank has in the long run no interest one way or the other, the bank has an exceptional interest in *refaits*. A *refait trente-et-un* at once gives the bank a certainty of winning the least sum staked in the two compartments, and an equal chance of winning the larger sum instead. Any *refait* gives the bank the chance that on a new trial a *refait trente-et-un* may be made; and though this chance (that is, the chance that there will first be a common *refait* and then a *refait trente-et-un*) is small, it tells in the long run and must be added to the advantage obtained from the chance of a *refait trente-et-un* at once.

Now it may seem as though the bank would gain very little from so small an advantage. A *refait* may occur tolerably often in any long series of trials, but a *refait trente-et-un* only at long intervals. It is only one out of ten different *refaits*, which to the uninitiated seem all equally likely to occur; so that he supposes the chance of a *refait trente-et-un* to be only one-tenth of the chance (itself small at each trial) that there will be a *refait* of some sort. But, to begin with, this supposition is incorrect. Calculation shows that the chance of a *refait* of some sort occurring is 1,097 in 10,000, or nearly one in nine. The chance of a *refait trente-et-un* is not one-tenth of this, or about 110 in 10,000, but

219 in 10,000, or twice as great as the uninitiated imagine. Thus in very nearly two games in 91, instead of one game in 91, a *refait trente-et-un* occurs. It follows from this, combined with the circumstance that on the average the bank wins half its stakes only in the case of one of these *refaits* (and account being also taken of the slight subordinate chance above mentioned), that the mathematical advantage of the bank is very nearly one-ninetieth of all the sums deposited. The actual percentage is $1\frac{1}{1\frac{1}{0}}$ *per deposit*, or 1*l.* 2*s.* per 100*l.* And in passing it may be noticed as afford ng good illustration of the mistakes the uninitiated are apt to make in such matters, that if instead of the *refait trente-et-un* the bankers took to themselves the *refait quarante*, then, instead of this percentage per deposit, the percentage would be only $\frac{3}{2\frac{3}{0}}$, or 3*s.* per 100*l.*

But even an average advantage of 1*l.* 2*s.* per 100*l.* on each deposit made by the bank is thought by the frequenters of the table to be very slight. It makes the odds against the players about 913 to 892 on each trial, and the difference seems trifling. On considering the probable results of a year's play, however, we find that the bankers could obtain tremendous interest for a capital which would make them far safer against ruin than is thought necessary in any ordinary mercantile business. Suppose play went on upon only 100 evenings in each year; that each evening 100 games were played; and that on each game the total sum risked on both *rouge* and *noir* was 50*l.* Then the total sum deposited by the bank (very much exceeding the total sum *risked*,

which on each game is only the difference between the sums staked on *rouge* and on *noir*) would be 500,000*l.*; and $1\frac{1}{19}$ per cent. on this sum would be 5,500*l.* I follow De Morgan in taking these numbers, which are far below what would generally be deposited in 100 evenings of play. Now, it can be shown that if the bankers started with such a sum as 5,500*l.*, they would be practically safe from all chance of ruin. So that in 100 playing nights they would probably make cent. per cent. on their capital. In places where gambling is encouraged they could readily in a year make 300 per cent. on their capital at the beginning of the year.

De Morgan points out that, though the editor of Hoyle does not correctly estimate the chances in this game, underrating the bank's advantage; yet, even with this erroneous estimate, the gains per annum on a capital of 5,500*l.* would be 12,000*l.* (instead of 16,500*l.* as when properly calculated). As he justly says, 'the preceding results, or either of them, being admitted, it might be supposed hardly necessary to dwell upon the ruin which must necessarily result to individual players against a bank which has so strong a chance of success against its united antagonists.' 'But,' he adds, 'so strangely are opinions formed upon this subject, that it is not uncommon to find persons who think they are in possession of a specific by which they must infallibly win.' If both the banker and the player staked on each game 1-160th part of their respective funds, and the play was to continue till one or other side was ruined, the bank would have 49 chances to 1 in its

favour against that one player. But if, as more commonly is the case, the player's stake formed a far larger proportion of his property, these odds would be immensely increased. If a player staked one-tenth of his money on each game against the same sum, supposed to be 1-160th of the bank's money, the chances would be 223 to 1 that he would be ruined if he persisted long enough. In other words, his chance of escaping ruin would be the same as that of drawing one single marked ball out of a bag containing 224.

Other games played at the gaming-tables, however different in character they may be from *rouge-et-noir*, give no better chances to the players. Indeed, some games give far inferior chances. There is not one of them at which any system of play can be safe in the long run. If the system is such that the risk on each venture is small, then the gains on each venture will be correspondingly small. Many ventures, therefore, must be made in order to secure any considerable gains; and when once the number of ventures is largely increased, the small risk on each becomes a large risk, and if the ventures be very numerous becomes practically a certainty of loss. On the other hand there are modes of venturing which, if successful once only, bring in a large profit; but they involve a larger immediate risk.

In point of fact, the supposition that any system can be devised by which success in games of chance may be made certain, is as utterly unphilosophical as faith in the invention of perpetual motion. That the

supposition has been entertained by many who have passed all their lives in gambling proves only—what might also be safely inferred from the very fact of their being gamblers—that they know nothing of the laws of probability. Many men who have passed all their lives among machinery believe confidently in the possibility of perpetual motion. They are familiar with machinery, but utterly ignorant of mechanics. In like manner, the life-long gambler is familiar with games of chance, but utterly ignorant of the laws of chance.

It may appear paradoxical to say that chance results right themselves—nay, that there is an absolute certainty that in the long run they will occur as often (in proportion) as their respective chances warrant, and at the same time to assert that it is utterly useless for any gambler to trust to this circumstance. Yet not only is each statement true, but it is of first-rate importance in the study of our subject that the truth of each should be clearly recognised.

That the first statement is true, will perhaps not be questioned. The reasoning on which it is based would be too abstruse for these pages; but it has been experimentally verified over and over again. Thus, if a coin be tossed many thousands of times, and the numbers of resulting 'heads' and 'tails' be noted, it is found, not necessarily that these numbers differ from each other by a very small quantity, but that their difference is small compared with either. In mathematical phrase, the two numbers are nearly in a ratio of equality. Again, if a dice be tossed, say, six million times, then, although

there will not probably have been exactly a million throws of each face, yet the number of throws of each face will differ from a million by a quantity very small indeed compared with the total number of throws. So certain is this law, that it has been made the means of determining the real chances of an event, or of ascertaining facts which had been before unknown. Thus, De Morgan relates the following story in illustration of this law. He received it 'from a distinguished naval officer, who was once employed to bring home a cargo of dollars.' 'At the end of the voyage,' he says, 'it was discovered that one of the boxes which contained them had been forced; and on making further search a large bag of dollars was discovered in the possession of some one on board. The coins in the different boxes were a mixture of all manner of dates and sovereigns; and it occurred to the commander, that if the contents of the boxes were sorted, a comparison of the proportions of the different sorts in the bag, with those in the box which had been opened, would afford strong presumptive evidence one way or the other. This comparison was accordingly made, and the agreement between the distribution of the several coins in the bag and those in the box was such as to leave no doubt as to the former having formed a part of the latter.' If the bag of stolen dollars had been a small one the inference would have been unsafe, but the great number of the dollars corresponded to a great number of chance trials; and as in such a large series of trials the several results would be sure to occur in numbers corresponding

to their individual chances, it followed that the number of coins of the different kinds in the stolen lot would be proportional, or very nearly so, to the number of those respective coins in the forced box. Thus, in this case the thief increased the strength of the evidence against him by every dollar he added to his ill-gotten store.

We may mention, in passing, an even more curious application of this law, to no less a question than that much-talked of but little understood problem, the squaring of the circle. It can be shown by mathematical reasoning, that, if a straight rod be so tossed at random into the air as to fall on a grating of equidistant parallel bars, the chance of the rod falling through depends on the length and thickness of the rod, the distance between the parallel bars, *and* the proportion in which the circumference of a circle exceeds the diameter. So that when the rod and grating have been carefully measured, it is only necessary to know the proportion just mentioned in order to calculate the chance of the rod falling through. But also, if we can learn in some other way the chance of the rod falling through, we can infer the proportion referred to. Now the law we are considering teaches us that if we only toss the rod often enough, the chance of its falling through will be indicated by the number of times it actually does fall through, compared with the total number of trials. Hence we can estimate the proportion in which the circumference of a circle exceeds the diameter by merely tossing a rod over a grating several

thousand times, and counting how often it falls through. The experiment has been tried, and Professor De Morgan tells us that a very excellent evaluation of the celebrated proportion (the determination of which is equivalent in reality to squaring the circle) was the result.

And let it be noticed, in passing, that this inexorable law—for in its effects it is the most inflexible of all the laws of probability—shows how fatal it must be to contend long at any game of pure chance, where the odds are in favour of our opponent. For instance, let us assume for a moment that the assertion of the foreign gaming bankers is true, and that the chances are but from $1\frac{1}{4}$ to $2\frac{1}{2}$ per cent. in their favour. Yet in the long run, this percentage must manifest its effects. Where a few hundreds have been wagered the bank may not win $1\frac{1}{4}$ or $2\frac{1}{2}$ on each, or may lose considerably; but where thousands of hundreds are wagered, the bank will certainly win about their percentage, and the players will therefore lose to a corresponding extent. This is inevitable, so only that the play continue long enough. Now, it is sometimes forgotten that to ensure such gain to the bank, it is by no means necessary that the players should come prepared to stake so many hundreds of pounds. Those who sit down to play may not have a tithe of the sum necessary—if only wagered once—to ensure the success of the bank. But every florin the players bring with them may be, and commonly is, wagered over and over again. There is repeated gain and loss, and loss and gain; insomuch that the player who finally loses a hundred pounds, may have wagered

in the course of the sitting a thousand or even many thousand pounds. Those fortunate beings who 'break the bank' from time to time, may even have accomplished the feat of wagering millions during the process which ends in the final loss of the few thousands they may have begun with.

Why is it, then, it will be asked, that this inexorable law is yet not to be trusted? For this reason, simply, that the mode of its operation is altogether uncertain. If in a thousand trials there has been a remarkable preponderance of any particular class of events, it is not a whit more probable that the preponderance will be compensated by a corresponding deficiency in the next thousand trials than that it will be repeated in that set also. The most probable result of the second thousand trials is precisely that result which was most probable for the first thousand—that is, that there will be no marked preponderance either way. But there *may be* such a preponderance; and it may lie either way. It is the same with the next thousand, and the next, and for every such set. They are in no way affected by preceding events. In the nature of things, how can they be? But, 'the whirligig of time brings in its revenges' in its own way. The balance is restored just as chance directs. It may be in the next thousand trials, it may be not before many thousands of trials. We are utterly unable to guess when or how it will be brought about.

But it may be urged that this is mere assertion; and many will be very ready to believe that it is opposed to

experience, or even contrary to common sense. Yet experience has over and over again confirmed the matter, and common sense, though it may not avail to unravel the seeming paradox, yet cannot insist on the absurdity that coming events of pure chance are affected by completed events of the same kind. If a person has tossed 'heads' nine times running (we assume fair and lofty tosses with a well-balanced coin), common sense teaches him, as he is about to make the tenth trial, that the chances on that trial are precisely the same as the chances on the first. It would, indeed, have been rash for him to predict that he would reach that trial without once failing to toss 'head'; but as the thing has happened, the odds originally against it count for nothing. They are disposed of by known facts. We have said, however, that experience confirms our theory. It chances that a series of experiments have been made on coin-tossing. Buffon was the experimenter, and he tossed thousands of times, noting always how many times he tossed 'head' running before 'tail' appeared. In the course of these trials he many times tossed 'head' nine times running. Now, if the tossing 'head' nine times running rendered the chance of tossing a tenth head much less than usual, it would necessarily follow that in considerably more than one-half of these instances Buffon would have failed to toss a tenth head. But he did not. In about half the cases in which he tossed nine 'heads' running, the next trial also gave him 'head'; and about half of these tossings of ten suc-

cessive 'heads' were followed by the tossing of an eleventh 'head.' In the nature of things this was to be expected.

And now let us consider the cognate questions suggested by our sharper's ideas respecting the person who plays. This person is to consider carefully whether he is '*in vein*,' and not otherwise to play. He is to be cool and businesslike, for fortune is invariably adverse to an angry player. Steinmetz, who appears to place some degree of reliance on the suggestion that a player should be 'in vein,' cites in illustration and confirmation of the rule the following instance from his own experience:—'I remember,' he says, 'a curious incident in my childhood which seems very much to the point of this axiom. A magnificent gold watch and chain were given towards the building of a church, and my mother took three chances, which were at a very high figure, the watch and chain being valued at more than 100*l*. One of these chances was entered in my name, one in my brother's, and a third in my mother's. I had to throw for her as well as myself. My brother threw an insignificant figure; for myself I did the same; but, oddly enough, I refused to throw for my mother on finding that I had lost my chance, saying that I should wait a little longer—rather a curious piece of prudence' (read, rather, superstition) 'for a child of thirteen. The raffle was with three dice; the majority of the chances had been thrown, and thirty-four was the highest.' (It is to be presumed that the three dice were thrown twice, yet 'thirty-four' is

a remarkable throw with six dice, and 'thirty-six' altogether exceptional.) 'I went on throwing the dice for amusement, and was surprised to find that every throw was better than the one I had in the raffle. I thereupon said, "Now I'll throw for mamma." I threw thirty-six, which won the watch! My mother had been a large subscriber to the building of the church, and the priest said that my winning the watch for her was quite *providential*. According to M. Houdin's authority, however, it seems that I only got into "vein"—but how I came to pause and defer throwing the last chance has always puzzled me respecting this incident of childhood, which made too great an impression ever to be effaced.'

It is probable that most of my readers can recall some circumstance in their lives, some surprising coincidence, which has caused a similar impression, and which they have found it almost impossible to regard as strictly fortuitous.

In chance games especially, curious coincidences of the sort occur, and lead to the superstitious notion that they are not mere coincidences, but in some definite way associated with the fate or fortune of the player, or else with some event which has previously taken place—a change of seats, a new deal, or the like. There is scarcely a gambler who is not prepared to assert his faith in certain observances whereby, as he believes, a change of luck may be brought about. In an old work on card-games the player is gravely advised, if the luck has been against him, to turn three times round with

his chair, 'for then the luck will infallibly change in your favour.'

Equally superstitious is the notion that anger brings bad luck, or, as M. Houdin's authority puts it, that 'the demon of bad luck invariably pursues a passionate player.' At a game of pure chance good temper makes the player careless under ill-fortune, but it cannot secure him against it. In like manner, passion may excite the attention of others to the player's losses, and in any case causes himself to suffer more keenly under them, but it is only in this sense that passion is unlucky for him. He is as likely to make a lucky hit when in a rage as in the calmest mood.

It is easy to see how superstitions such as these take their origin. We can understand that since one who has been very unlucky in games of pure chance, is not antecedently likely to continue equally unlucky, a superstitious observance is not unlikely to be followed by a seeming change of luck. When this happens the coincidence is noted and remembered; but failures are readily forgotten. Again, if the fortunes of a passionate player be recorded by dispassionate bystanders, he will not appear to be pursued by worse luck than his neighbours; but he will be disposed to regard himself as the victim of unusual ill-fortune. He may perhaps register a vow to keep his temper in future; and then his luck may seem to him to improve, even though a careful record of his gains and losses would show no change whatever in his fortunes.

But it may not seem quite so easy to explain those

undoubted runs of luck by which players 'in the vein' (as supposed) have broken gaming-banks, and have enabled those who have followed their fortunes to achieve temporary success. The history of the notorious Garcia, and of others who like him have been for awhile the favourites of fortune, will occur at once to many of my readers, and will appear to afford convincing proof of the theory that the luck of such gamesters has had a real influence on the fortunes of the game. The following narrative gives an accurate and graphic picture of the way in which these 'bank-breakers' are followed and believed in, while their success seems to last.

The scene is laid in one of the most celebrated German Kursaals.

'What a sudden influx of people into the room! Now, indeed, we shall see a celebrity. The tall light-haired young man coming towards us, and attended by such a retinue, is a young Saxon nobleman who made his appearance here a short time ago, and commenced his gambling career by staking very small sums; but, by the most extraordinary luck, he was able to increase his capital to such an extent that he now rarely stakes under the maximum, and almost always wins. They say that when the croupiers see him place his money on the table, they immediately prepare to pay him, without waiting to see which colour has actually won, and that they have offered him a handsome sum down to desist from playing while he remains here. Crowds of people stand outside the Kursaal doors every

morning, awaiting his arrival, and when he comes following him into the room, and staking as he stakes. When he ceases playing they accompany him to the door, and shower on him congratulations and thanks for the good fortune he has brought them. See how all the people make way for him at the table, and how deferential are the subdued greetings of his acquaintances! He does not bring much money with him, his luck is too great to require it. He takes some notes out of a case, and places maximums on *black* and *couleur*. A crowd of eager hands are immediately outstretched from all parts of the table, heaping up silver and gold and notes on the spaces on which he has staked his money, till there scarcely seems room for another coin, while the other spaces on the table only contain a few florins staked by sceptics who refuse to believe in the count's luck.' He wins; and the narrative proceeds to describe his continued successes, until he rises from the table a winner of about one hundred thousand francs at that sitting.

The success of Garcia was so remarkable at times as to affect the value of the shares in the *Privilegirte Bank* ten or twenty per cent. Nor would it be difficult to cite many instances which seem to supply incontrovertible evidence that there is something more than common chance in the temporary successes of these (so-called) fortunate men.

Indeed, to assert merely that in the nature of things there can be no such thing as luck that can be depended on even for a short time, would probably be quite

useless. There is only one way of meeting the infatuation of those who trust in the fates of lucky gamesters. We can show that, granted a sufficient number of trials—and it will be remembered that the number of those who have risked their fortunes at *roulette* and *rouge-et-noir* is incalculably great—there must *inevitably* be a certain number who appear exceptionally lucky; or, rather, that the odds are overwhelmingly against the continuance of play on the scale which prevails at the foreign gambling-tables, without the occurrence of several instances of persistent runs of luck.

To remove from the question the perplexities resulting from the nature of the above-named games, let us suppose that the tossing of a coin is to determine the success or failure of the player, and that he will win if he throws 'head.' Now if a player tossed 'head' twenty times running on any occasion it would be regarded as a most remarkable run of luck, and it would not be easy to persuade those who witnessed the occurrence that the thrower was not in some special and definite manner the favourite of Fortune. We may take such exceptional success as corresponding to the good fortune of a 'bank-breaker.' Yet it is easily shown that with a number of trials which must fall enormously short of the number of cases in which fortune is risked at foreign Kursaals, the throwing of twenty successive 'heads' would be practically *insured*. Suppose every adult person in Britain—say 10,000,000 persons in all—were to toss a coin, each tossing until 'tail' was thrown; then it is practically certain that

several among them would toss twenty times before 'tail' was thrown. Thus: It is certain that about five millions would toss 'head' once; of these about one-half, or some two millions and a half, would toss 'head' on the second trial; about a million and a quarter would toss 'head' on the third trial; about six hundred thousand on the fourth; some three hundred thousand on the fifth; and by proceeding in this way—roughly halving the numbers successively obtained—we find that some eight or nine of the ten million persons would be almost certain to toss 'head' twenty times running. It must be remembered that so long as the numbers continue large the probability that *about* half will toss 'head' at the next trial amounts almost to certainty. For example, about 140 toss 'head' sixteen times running: now, it is utterly unlikely that of these 140, fewer than sixty will toss 'head' yet a seventeenth time. But if the above process failed on trial to give even one person who tossed 'heads' twenty times running—an utterly improbable event—yet the trial could be made four or five times, with practical certainty that not one or two, but thirty or forty, persons would achieve the seemingly incredible feat of tossing 'head' twenty times running. Nor would all these thirty or forty persons fail to throw even three or four more 'heads.'

Now, if we consider the immense number of trials made at gambling-tables, and if we further consider the gamblers as in a sense typified by our ten millions of coin-tossers, we shall see that it is not merely

probable but absolutely certain that from time to time there must be marvellous runs of luck at *roulette*, *rouge-et-noir*, *hazard*, *faro*, and other games of chance. Suppose that at the public gaming-tables on the Continent there sit down each night but one thousand persons in all, that each person makes but ten ventures each night, and that there are but one hundred gambling nights in the year—each supposition falling far below the truth—there are then one million ventures each year. It cannot be regarded as wonderful, then, that among the fifty millions of ventures made (on this supposition) during the last half century, there should be noted some runs of luck which on any single trial would seem incredible. On the contrary, this is so far from being wonderful that it would be far more wonderful if no such runs of luck had occurred. It is probable that if the actual number of ventures, and the circumstances of each, could be ascertained, and if any mathematician could deal with the tremendous array of figures in such sort as to deduce the exact mathematical chance of the occurrence of bank-breaking runs of luck, it would be found that the antecedent odds were many millions to one in favour of the occurrence of a certain number of such events. In the simpler case of our coin-tossers the chance of twenty successive 'heads' being tossed can be quite readily calculated. I have made the calculation, and I find that if the ten million persons had each two trials the odds would be more than 10,000 to 1 in favour of the occurrence of twenty successive

'heads' once at least; and only a million and a half need have a single trial each, in order to give an even chance of such an occurrence.

But we may learn a further lesson from our illustrative tossers. We have seen that granted only a sufficient number of trials, runs of luck are practically certain to occur: but we may also infer that no run of luck can be *trusted* to continue. The very principle which has led us to the conclusion that several of our tossers would throw twenty 'heads' successively, leads also to the conclusion that one who has tossed 'heads' twelve or thirteen times, or any other considerable number of times in succession, is not more (or less) likely to toss 'head' on the next trial than at the beginning. *About half*, we said, in discussing the fortunes of the tossers, would toss 'head' at the next trial: in other words, *about half* would fail to toss 'head.' The chances for and against these lucky tossers are equal at the next trial, precisely as the chances for and against the least lucky of the ten million tossers would be equal at any single tossing.

Yet, it may be urged, experience shows that luck continues; for many have won by following the lead of lucky players. Now I might, at the outset, point out that this belief in the continuance of luck is suggested by an idea directly contradictory to that on which is based the theory of the 'maturity of the chances.' If the oftener an event has occurred, the more unlikely is its occurrence at the next trial—the common belief—then, contrary to the common

belief, the oftener a player has won (that is, the longer has been his run of luck), the more unlikely is he to win at the next venture. We cannot separate the two theories, and assume that the theory of the maturity of the chances relates to the play, and the theory of runs of luck to the player. The success of the player at any trial is as distinctly an event—a chance event—as the turning up of ace or deuce at the cast of a die.

What then are we to say of the experience of those who have won money by following a lucky player? Let us revert to our coin-tossers. Let us suppose that the progress of the venture in a given county is made known to a set of betting men in that county; and that when it becomes known that a person has tossed 'head' twelve times running, the betting men hasten to back the luck of that person. Further, suppose this to happen in every county in England. Now we have seen that these persons are no more likely to toss a thirteenth 'head,' than they are to fail. About half will succeed and about half will fail. Thus about half their backers will win and about half will lose. But the successes of the winners will be widely announced; while the mischances of the losers will be concealed. This will happen—the like notoriously does happen—for two reasons. First, gamblers pay little attention to the misfortunes of their fellows: the professed gambler is utterly selfish, and moreover he hates the sight of misfortune because it unpleasantly reminds him of his own risks. Secondly, losing gamblers do

not like their losses to be noised abroad; they object to having their luck suspected by others, and they are even disposed to blind themselves to their own ill-fortune as far as possible. Thus, the inevitable success of about one-half of our coin-tossers would be accompanied inevitably by the success of those who 'backed their luck,' and the successes of such backers would be bruited abroad and be quoted as examples; while the failure of those who had backed the other half (whose luck was about to fail them), would be comparatively unnoticed. Unquestionably the like holds in the case of public gambling-tables. If any doubt this, let them inquire what has been heard of those who continued to back Garcia and other 'bank-breakers.' We know that Garcia and the rest of these lucky gamblers have been ruined; they had risen too high and were followed too constantly for their fall to remain unnoticed. But what has been heard of those unfortunates who backed Garcia after his last successful evening, and before the change in his luck had been made manifest? We hear nothing of them, though a thousand stories are told of those who made money while Garcia and the rest were 'in luck.'

In passing, we may add to these considerations the circumstance that it is the interest of gaming-bankers to conceal the misfortunes of the unlucky, and to announce and exaggerate the success of the fortunate.

I by no means question, be it understood, the possibility that money may be gained quite safely by gambling. Granting, first, odds such as the 'banks'

have in their favour; secondly, a sufficient capital to prevent premature collapse; and thirdly, a sufficient number of customers, success is absolutely certain in the long run. The capital of the gambling-public doubtless exceeds collectively the capital of the gambling-banks; but it is not used collectively: the fortunes of the gambling-public are devoured successively, the sticks which would be irresistible when combined, are broken one by one. I leave my readers to judge whether this circumstance should encourage gambling or the reverse.

I may thus present the position of the gambler who is not ready to secure Fortune as his ally by trickery:—If he meets gamblers who are not equally honest, he is not trying his luck against theirs, but at the best (as De Morgan puts it) only a part of his luck against the whole of theirs; if he meets players as honest as himself, he must nevertheless, as Lord Holland said to Selwyn, 'be in earnest and without irony —en vérité le serviteur très-humble des événements— in truth, the very humble servant of events.'

FAIR AND UNFAIR WAGERS.

I GAVE in my 'How to Play Whist' (under the head 'Whist Whittlings') a case in which a certain man of title used to offer freely 1,000*l.* to 1*l.* against the occurrence of a whist hand containing no card above a nine —a most unfair wager. Odds of a thousand pounds to one are very tempting to the inexperienced. 'I risk my pound,' such a one will say, 'but no more, and I may win a thousand.' That is the *chance*; and what is the *certainty*? The certainty is that in the long run such bets will involve a loss of 1,828*l.* for each thousand pounds gained, or a net loss of 828*l.* As certain to all intents as that two and two make four, a large number of wagers made on this plan would mean for the clever layer of the odds a very large gain. Yet Lord Yarborough would probably have been indignant to a degree if he had been told that in taking 1*l.* for each hand on which he wagered which did not prove to be a 'Yarborough,' he was in truth defrauding the holder of the hand of 9*s.* $0\frac{3}{4}$*d.*, notwithstanding the preliminary agreement, simply because the preliminary agreement was an unfair one. As to his being told that even if he had wagered 1,828*l.*

against 1*l.* the transaction would have been intrinsically immoral, doubtless he and his opponent would equally have scouted the idea.

A curious instance of the loss of all sense of honour, or even honesty, which betting begets, occurred to me when I was in New Zealand. A bookmaker ('by profession,' as he said), as genial and good-natured a man as one would care to meet, and with a strong sense of right and justice outside betting, had learned somehow that ten horses can come in (apart from dead heats) in 3,628,800 different ways. This curious piece of information seemed to him an admirable way of gaining money from the inexperienced. So he began to wager about it, endeavouring—though, as will be seen, he failed—to win money by wagering on a certainty. Unfortunately, he came early across a man as cute as himself and a shade cuter (*à brigand brigand et demi*), who worded the question on which the wager turns thus:—'In how many ways can ten horses be placed?' Of course, this is a very different thing. Only the first three horses can be placed, and the sets of three which can be made out of ten horses number only 10 times 9 times 8, or 720 (there are only 120 actual sets of three, but each set can be placed in six different ways). My genial, but (whatever he thought himself) not quite honest friend, submitted the matter to me. Not noticing, at first, the technical use of the word 'placed,' I told him there were 3,628,800 different arrangements: he rejoiced as though the money wagered were already in his pocket. When

this was corrected, and I told him his opponent had certainly won, as the question would be understood by betting men, he was at first depressed; but presently recovering, he said, 'Ah, well; I shall win more out of this little trick, now I see through it, than I lose this time.'

It is well to have some convenient standard of reference, not only as respects the fairness or unfairness of betting transactions, but as to the true nature of the chances involved or supposed to be involved. Many men bet on horse races without any clear idea of the chances they are really running. To see that this is so, it is only necessary to notice the preposterous way in which many bettors combine their bets. I do not say that many, even among the idiots who wager on horses they know nothing about, would lay heavier odds against the winning of a race by one of two horses than he would lay against the chance of either horse separately; but it is quite certain that not one bettor in a hundred knows either how to combine the odds against two, three, or more horses, so as to get the odds about the lot, or how to calculate the chance of double, triple, or multiple events. Yet these are the very first principles of betting; and a man who bets without knowing anything about such matters runs as good a chance of ultimate success as a man who, without knowing the country, should take a straight line in the hunting-field.

Now, apart from what may be called roguery in horse-racing, every bet in a race may be brought into direct comparison with the simple and easily understood

chance of success in a lottery where there is a single prize, and therefore only one prize ticket: and the chance of the winner of a race, where several horses run, being one particular horse, or one of any two, three, or more horses, can always be compared with the easily understood chance of drawing a ball of one colour out of a vase containing so many balls of that colour and so many of another. So also can the chance of a double or triple event be compared with a chance of the second kind.

Let us first, then, take the case of a simple lottery, and distinguish between a fair lottery and an unfair one. Every actual lottery, I remark in passing, is an unfair one; at least, I have never yet heard of a fair one, and I can imagine no possible case in which it would be worth anyone's while to start a fair lottery.

Suppose ten persons each contribute a sovereign to form a prize of 10*l.*; and that each of the ten is allowed to draw one ticket from among ten, one marked ticket giving the drawer the prize. That is a fair lottery; each person has paid the right price for his chance. The proof is, that if anyone buys up all the chances at the price, thus securing the certainty of drawing the marked ticket, he obtains as a prize precisely the sum he has expended.

This, I may remark, is the essential condition for a fair lottery, whatever the number of prizes; though we have no occasion to consider here any case except the very simple case of a one-prize lottery. Where there are several prizes, whether equal or unequal in value,

we have only to add their value together: the price for all the tickets together must equal the sum we thus obtain. For instance, if the ten persons in our illustrative case, instead of marking one ticket were to mark three, for prizes worth 5*l.*, 3*l.*, and 2*l.*, the lottery would be equally fair. Anyone, by buying up all the ten tickets, would be sure of all three prizes, that is, he would pay ten pounds and get ten pounds—a fair bargain.

But suppose, reverting to one-prize lotteries, that the drawer of the marked ticket were to receive only 8*l.* instead of 10*l.* as a prize. Then clearly the lottery would be unfair. The test is, that a man must pay 10*l.* to insure the certainty of winning the prize of 8*l.*, and will then be 2*l.* out of pocket. So of all such cases. When the prize, if there is but one, or the sum of all the prizes together, if there are several, falls short of the price of all the tickets together, the lottery is an unfair one. The sale of each ticket is a swindle; the total amount of which the ticket-purchasers are swindled being the sum by which the value of the prize or prizes falls short of the price of the tickets.

We see at once that a number of persons in a room together would never allow an unfair lottery of this sort. If each of the ten persons put a sovereign into the pool, each having a ticket, the drawer of the prize ticket would be clearly entitled to the pool. If one of the ten started the lottery, and if when the 10*l.*, including his own, has been paid in to the pool, he proposed to take charge of the pool, and to pay 8*l.* to the drawer of the

marked ticket, it would be rather too obvious that he was putting 2*l.* in his pocket. But lotteries are not conducted in this simple way, or so that the swindle becomes obvious to all engaged. As a matter of fact, all lotteries are so arranged that the manager or managers of the lottery put a portion of the proceeds (or pool) into their pockets. Otherwise it would not be worth while to start a lottery. Whether a lottery is started by a nation, or for a cause, or for personal profit, it always is intended for profit; and profit is always secured, and indeed can only be secured, by making the total value of the prizes fall short of the sum received for the tickets.

I would not be understood to say that I regard all unfair lotteries as swindles. In the case of lotteries for a charitable purpose I suppose the object is to add gambling excitement to the satisfaction derived from the exercise of charity. The unfairness is understood and permitted; just as, at a fancy fair, excessive prices are charged, change is not returned, and other pleasantries are permitted which would be swindles if practised in real trading. But in passing I may note that even lotteries of this kind are objectionable. Those who arrange them have no wish to gain money for themselves; and many who buy tickets have no wish to win prizes, and would probably either return any prize they might gain or pay its full value. But it is not so with all who buy tickets; and even a charitable purpose will not justify the mischief done by the encouragement of the gambling spirit of such persons. In nearly all cases

the money gained by such lotteries might, with a little more trouble but at less real cost, be obtained directly from the charitably minded members of the community.

To return, however, to my subject.

I have supposed the case of ten persons gambling fairly in such a way that each venture made by the ten results in a single-prize lottery. But as we know, a betting transaction is nearly always arranged between two persons only. I will therefore now suppose only two persons to arrange such a lottery, in this way :— The prize is 10*l.*, as before, and there are ten tickets; one of the players, A, puts, say, 3*l.* in the pool, while the other, B, puts 7*l.*; three tickets are marked as winning tickets; A then draws at random once only; if he draws a marked ticket, he wins the pool; if he draws an unmarked ticket, B takes the pool. This is clearly fair; in fact it is only a modification of the preceding case. A takes the chances of three of the former players, while B takes the chances of the remaining seven. True, there seems to be a distinction. If we divided the former ten players into two sets, one of three, the other of seven, there would not be a single drawing to determine whether the prize should go to the three or to the seven; each of the ten would draw a ticket, all the tickets being thus drawn. Yet in reality the methods are in principle precisely the same. When the ten men have drawn their tickets in the former method, three tickets have been assigned at random to the three men and seven tickets to the other seven; and the chance that the three have won is the

chance that one of the three tickets is the marked one. In the latter method there are ten tickets, of which three are marked; and the chance that A wins the prize is the chance that at his single drawing he takes one of the three marked tickets. But obviously the chance that a certain marked ticket in ten is one of the three taken at random must be exactly the same as the chance that a certain ticket taken at random from among the ten is one of three marked tickets; for each of these chances is clearly three times as good as the chance of drawing, at a single trial, one particular ticket out of ten.

It will be found that we can now test any wager, not merely determining whether it is fair or unfair, but the extent to which it is so, if only the actual chance of the horse or horses concerned is supposed to be known. Unfortunately, in the great majority of cases bets are unfair in another way than that which we are for the moment considering, the odds not only differing from those fairly representing the chances of the horse or horses concerned, but one party to the wager having better knowledge than the other what those chances are. Cases of this kind will be considered further on.

Suppose that the just odds against a horse in a race are 9 to 1. By this I mean that so far as the two bettors are concerned (that is, from all that they know about the chances of the horse), it is nine times more likely that the horse will not win the race than that he will. Now, it is nine times more likely that a particular ticket among ten will not be drawn at a single trial

than that it will. So the chance of this horse is correctly represented by the chance of the prize ticket being drawn in a lottery where there are ten tickets in all. If two persons arrange such a lottery, and A pays in 1*l.* to the pool, while the other, B, pays in 9*l.*, making 10*l.* in all, A gets a fair return for his money in a single drawing, one ticket out of the ten being marked for the prize. A represents, then, the backer of the horse who risks 1*l.*; B the layer of the odds who risks 9*l.* The sum of the stakes is the prize, or 10*l.* If A risks less than 1*l.*, while B risks 9*l.*, the total prize is diminished; or if, while A risks 1*l.*, B risks less than 9*l.*, the total is diminished. In either case the wrong done to the other bettor amounts precisely to the amount by which the total is diminished. If, for instance, A only wagered 18*s.* against B's 9*l.*, the case is exactly the same as though A and B having severally contributed 1*l.* and 9*l.* to a pool, one ticket out of ten having been marked and A to have one chance only of drawing it (which we have just seen would be strictly fair), A abstracted two shillings from the pool. If B only wagered 7*l.* instead of 9*l.* against A's 1*l.* the case would be just the same as though, after the pool had been made up as just described, B had abstracted 2*l.*

Take another case. The odds are 7 to 3 against a horse. The chance of its winning is the same as that of drawing a marked ticket out of a bag containing ten, when three are marked and seven are unmarked. We know that in this case two players, A and B, forming the lottery, must severally contribute 3*l.* and 7*l.* to the

pool, and if on a single drawing one of the three marked tickets appears, then A wins the pool, or 10*l.*, whereas B takes it if one of the seven unmarked tickets is drawn. If the backer of the horse, instead of wagering 3*l.*, wagered only 2*l.* against 7*l.*, he would be precisely in the position of a player A, who, having paid in his 3*l.* to the pool of 10*l.* in all, should abstract a pound therefrom. If the layer of the odds wagered only 5*l.* against 3*l.*, he would be in the position of a player B, who, having paid in his 7*l.* to the pool of 10*l.* in all, should abstract 2*l.* therefrom.

Or, if any difficulty should arise in the reader's mind from this way of presenting matters, let him put the case thus:—Suppose the sum of the stakes 10*l.*; then the odds being 7 to 3 against, the case is as though three tickets were marked for the prize and seven unmarked; and the two players ought therefore to contribute severally 3*l.* and 7*l.* to make up the 10*l.* If the 10*l.* is made up in any other way, there is unfairness; one player puts in too much, the other puts in too little. If one puts in 2*l.* 10*s.* instead of 3*l.*, the other puts in 7*l.* 10*s.* instead of 7*l.*, and manifestly the former has wronged the latter to the extent of 1*l.*, having failed to put in 10*s.* which he ought to have put in, and having got the other to put in 10*s.* which ought not to have been put in. This seems clearer, I find, to some than the other way of presenting the matter. But as, in reality, bets are not made in this way, the other way, which in principle is the same, is more convenient. Bettors do not take a certain sum of money

for the total of their stakes, and agree how much each shall stake towards that sum; but they bet a certain sum against some other sum. It is easy to take either of these to find out how much *ought* to be staked against it, and thus to ascertain to what extent the proper total of the stakes has been affected either in excess or defect. And we can get rid of any difficulty arising from the fact that according to the side we begin from we get either an excess or a defect, by beginning always from the side of the one who wagers at least as much as he should do, at the proper odds, whatever they may be.

As a general rule, indeed, the matter is a good deal simplified by the circumstance that fraudulent bettors nearly always lay the odds. It is easy to see why. In fact, one of the illustrative cases above considered has already probably suggested the reason to the reader. I showed that when the odds are 9 to 1 and only 7 to 1 is laid, in pounds, the fraud is the same as removing 2*l.* from a pool of 10*l.*; whereas with the same odds, backing the horse by 18*s.* instead of 1*l.*, corresponded to removing two shillings from such a pool. Now, if a fraudulent gambler had a ready hand in abstracting coins from a pool, and were playing with some one who did not count the money handed over to him when he won, it would clearly be the same thing to him whether he contributed the larger or smaller sum to the pool, for he would abstract as many coins as he could, and it would be so much clear gain. But if he could not get at the pool, and therefore could only cheat by omitting to contribute his fair share, it would manifestly be far

better for him to be the buyer of the larger share of the chances. If he bought nine tickets out of ten, he might put in 7*l.*, pretending to put in 9*l.*, and pocket 2*l.*; whereas if he only bought one ticket, he could only defraud his companion by a few shillings out of the price of that ticket. Now, this is the hardship under which the fraudulent bettor labours. He cannot, at least he cannot generally, get at the stakes themselves: or, which comes to the same thing, he must pay up in full when he loses, otherwise he has soon to give up his profitable trade. Of course he may levant without paying, but this is only to be adopted as a last resource; and fraudulent betting is too steadily remunerative to be given up for the value of a single robbery of the simpler kind. Thus the bettor naturally prefers laying the odds. He can keep so much more out of the larger sum which ought to be laid against a horse than he could out of the smaller sum with which the horse should be backed.

Then there is another circumstance which still more strongly encourages the fraudulent bettor to lay the odds. It is much easier for him to get his victims to back a horse than to bet against one. In the first place, the foolish folk who expect to make a fortune by betting, take fancies for a particular horse, while they are not so apt to take fancies against any particular horse. But secondly, and this is the chief reason of their mode of betting, they want to make a great and sudden gain at a small risk. They have not time, for the most part, to make many wagers on any given race;

and to wager large sums against two or three horses would involve a great risk for a small profit. This, then, they do not care to do; preferring to back some particular horse, or perhaps two or three, by which they risk a comparatively small sum, and may win a large one. As Mr. Plyant truly remarks in Hawley Smart's 'Bound to Win,' 'The public is dramatic in its fancies; the public has always a dream of winning a thousand to ten if it can raise the tenner. The public, Mr. Laceby, knows nothing about racing, but as a rule is wonderfully up in the story of Theodore's winning the Leger, after a hundred pounds to a walking-stick had been laid against him. The public is always putting down its walking-stick and taking to crutches in consequence. . . . What the public will back at the lists the last few days before the Derby would astonish you: they've dreams, and tips, and fancies about the fifty to one lot you couldn't imagine.' Is it to be wondered at that the public finds its tastes in this respect humoured by the bookmakers, when we remember that it is from just such wagers as the public like to make that the bookmaker can most readily obtain the largest slice of profit?

But we must not fall into the mistake of supposing that all the foolish folk who back horses at long odds necessarily lose. On the contrary, many of them win money—unfortunately for others, and often for themselves. It would be a very foolish thing to pay 1*l.* for one of ten tickets in a lottery where the single prize was only worth 9*l.* Yet some of the foolish fellows

who did this must win the prize, gaining 8*l.* by the venture. If many others were encouraged to repeat such a venture, or if he repeated it himself (inferring from his success that he was born under a lucky star), they and he would have reason to repent. He might, indeed, be lucky yet again; and perhaps more than once. But the more he won in that way, the more he would trust in his good luck; and in the long run he would be sure to lose, if all his ventures were of the same foolish kind as the first.

We see, however, that the foolish bettor in any given case is by no means certain to lose. Nor is the crafty bettor who takes advantage of him at all sure to win. A man might steal 2*l.* or 3*l.* from the pool, after making up 9*l.* out of the 10*l.*, in the case I have imagined, and yet lose, because his opponent might be fortunate enough to draw the single marked ticket, and so win the 7*l.* or 8*l.* left in the pool.

In reality, however, though quite possibly some among the foolish bettors not only win money but even keep what they win, refraining from trying their luck afresh, it must not be supposed that the fraudulent bettor exposes himself to the risk of loss in the long run. He plays a safe game. Every one of his bets is a partial swindle; yet in each he runs the risk of loss. His entire series of bets is a complete swindle, in which he runs no risk whatever of loss, but insures a certain gain. Let us see how this is done.

Suppose there are two horses in a race, A and B,

and that the betting is 3 to 1 against B. In other words, the chance of A winning is as the chance of drawing a marked ticket out of a bag containing four tickets of which three are marked, while B's chance of winning is as that of drawing the single unmarked ticket. In this case, as the odds are in favour of one horse, our bookmaker will have to do a little backing, which, preferably, he would avoid. In fact, a race such as this, that is, a match between two horses, is not altogether to the bookmaker's taste; and what he would probably do in this case would be to obtain special information in some underhand way about the horses, and bet accordingly. Supposing, however, that he cannot do this, poor fellow, let us see how he is to proceed to insure profit. The first thing is to decide on some amount which shall be staked over each horse; and the theoretically exact way—the mathematical manner—of swindling would be as follows:—Suppose that with some person a wager were made at the just odds in favour of A, in such sort that the stakes on both sides amounted, let us say, to 1,200*l.*; the fair wager would be 900*l.* to 300*l.* that A will win; our swindler, however, having found some greenhorn X, whom he can persuade to take smaller odds, takes his book and writes down quickly 800*l.* to 300*l.* in favour of A. He now finds some other greenhorn, Y, who is very anxious to back A, and having duly bewailed his misfortune in having no choice but to lay against a horse who is—so he says—almost certain to win, he asks and obtains the odds of 900*l.* to 200*l.* in favour of A; that is to say, he wagers 200*l.*

to 900*l.* against A. Let us see how his book stands. He has wagered—

800*l.* to 300*l.* with X, that A wins;
200*l.* to 900*l.* with Y, that B wins.

If A wins, he receives 300*l.* from X, and pays 200*l.* to Y, pocketing a balance of 100*l.* If B wins, he pays 800*l.* to X and receives 900*l.* from Y, pocketing equally 100*l.*

The system by which bookmakers win has great advantages over the plan formerly adopted at public gaming-houses, and probably adopted still, though less publicly. At the gaming-house the bankers did run some little risk. They were bound to win in the long run; but they might lose for a night or two, or might even have a tolerably long run of bad luck. But a judicious bookmaker can make sure of winning money on every great race. Of course, if the bookmakers like a little excitement—and they are men, after all, though they do make their own providence—they can venture a little more than the nothing they usually venture. For instance, instead of laying the odds against all the horses, they can lay against all but one, and back that one heavily. Then, if that horse wins, they 'skin the lamb,' in the pleasing language of their tribe. But the true path to success is that which I have indicated above, and they know it (or I would assuredly not have indicated it).

Still, in every depth there is a deeper still. In the cases hitherto considered I have supposed that the

chances of a horse really are what the public odds indicate. If they are not, it might be supposed that only the owner of the horse and a few friends, besides the trainer, jockey, and one or two other *employés*, would know of this. But, as a matter of fact, the bookmakers generally find out tolerably soon if anything is wrong with a horse, or if he has had a very good trial and has a better chance of winning than had till then been supposed. Before very long this knowledge produces its effect in bringing the horse to its true price, or near it. In the former case the horse is very diligently 'pencilled' by the bookmakers, and recedes step by step in the betting, till he is either at long odds or is no longer backed at any price. In the latter, the horse is as diligently backed, till he has reached short odds, taking his place among the favourites, or perhaps as first favourite.

But in either process—that of driving a horse to long odds, or that of installing him in a position among the favourites, according to the circumstances—a great deal of money is made and lost—made by those who know what has really happened, lost by those who do not. We may be tolerably sure it is not 'the public' which gains. It is to 'the professional,' naturally, that the information comes first, and he makes a handsome profit out of it, before the change in the betting shows the public what has happened.

Now here, unfortunately, we touch on a part of our subject which affects men who are not, in a proper sense of the word, 'bookmakers.' It is a singular circum-

stance—or rather it is not at all singular, but accords with multiplied experiences, showing how the moral nature gets warped by gambling transactions—that men who are regarded by the world, and regard themselves, as gentlemen, seem to recognise nothing dishonourable in laying wagers which they *know* not to accord with the real chances of a horse. A man who would scorn to note the accidental marks on the backs of playing cards, and still more to make such marks, will yet avail himself of knowledge just as unfair in horse-racing as a knowledge of the backs of certain cards would be in whist or écarté.

I have elsewhere cited as an illustration the use which Hawley Smart, in one of his novels ('Bound to Win'), makes of this characteristic of sporting men. It has been objected, somewhat inconsistently, that in the first place the novelist's picture is inaccurate, and in the second the use which the hero of that story makes of knowledge about his own horses was perfectly legitimate. As to the first point, I may remark that I do not need to read Hawley Smart's novels, or any novels, to be well assured that the picture is perfectly accurate, and that sporting men do make use of special knowledge about a horse's chances to make profitable wagers. As to the second point, I note that it well illustrates my own position, that gambling has the effect of darkening men's sense of right and wrong: it shows that many sporting men regard as legitimate what is manifestly unfair.

Not to go over ground already trodden, I turn to

another of Hawley Smart's lively tales, the hero of which is a much more attractive man than Harold Luxmore in 'Bound to Win'—Grenville Rose in 'A Race for a Wife.' He is not, for a wonder, a sporting hero; in everything but the racing arrangements, which he allows to be made in his name, he behaves much as a gentleman should, and manifestly he is intended to represent an English gentleman. He comes across information which shows that, by the action of an old form of tenure called 'right of heriot,' a certain horse which is the leading favourite for the Two Thousand can be claimed and so prevented from running. Of the direct use of this information, to free the heroine from a rascally sporting lawyer, nothing need be said but 'serve the fellow right.' Another use is, however, made of the knowledge thus obtained, and it is from this use that the novel derives its name. To a racing friend of his, a lawyer (like himself and the villain of the story), the hero communicates the secret. To him the racing friend addresses this impressive response:—'Look here, old fellow. Racing is business with me; if you're not in for a regular mare's nest, there's heaps of money to be made out of this . . . . don't whisper it to your carpet-bag till you've seen me again. I say this honestly, (!) with a view to doing my best for you.' What this best is presently appears. I need not follow the workings of the plot, nor tell the end of the story. All that answers my present purpose is to indicate the nature of the 'book' which the gentlemanly Dallison, Silky Dallison as his friends call him, succeeds in

making for himself and his equally gentlemanly friend on the strength of the 'tip' given by the latter. 'We now stand to win between us 10,170*l.* if Coriander wins the Two Thousand, and just quits if he loses; not a bad book, Grenville!' To which Grenville, nothing loth, responds, 'By Jove! no.' Yet every wager by which this result has been obtained, if rightly considered, was as certainly a fraud as a wager laid upon a throw with cogged dice. For, what makes wagers on such throws unfair, except the knowledge that with such dice a certain result is more likely than any other? and what essential difference is there between such knowledge about dice and special knowledge about a horse's chance in a race? The doctrine may not be pleasant to sporting gentlemen who have not considered the matter, but once duly considered there cannot be a doubt as to its truth: a wager made with an opponent who does not possess equally accurate information about the chances involved, is not a fair wager but a fraud. It is a fraud of the same kind as that committed by a man who wagers after the race, knowing what the event of the race has been; and it only differs from such a fraud in degree in the same sense that robbing a till differs from robbing a bank.

It may be argued that by the same reasoning good whist players defraud inferior players who play with them for equal stakes. But the cases are altogether different. Good whist players do not conceal their strength. Their skill is known; and if inferior players choose to play on equal terms, trusting in good luck to befriend them, they do it at their own risk. If a

parallel is to be sought from the whist-table, it would be rather derived from the case of two players who had privately arranged a system of signalling; for in such a case there is knowledge on one side which is not only wanting on the other side, but of the possession of which the other partners have no suspicion. No one would hesitate to call that swindling. Now take the case of one who knows that, as the result of a certain trial, a horse which is the favourite in a great race will take part in it, indeed, but will only do so to make running for a better horse. Until the time when the owner of the horses declares to win with the latter, such knowledge enables its possessor to accept safely all wagers in favour of the horse; and he knows perfectly well, of course, that not one such wager is offered him except by persons ignorant of the true state of the case. Even if such offers are made by bookmakers, whose profession is swindling, and though we may not have a particle of sympathy with such men when they lose in this way, the acceptance of such wagers is in no sense justified. Two wrongs do not, in this case more than in any other, make a right.

I have said that in every depth there is a deeper still. In the subject I am dealing with there is a deepest depth of all. I will not, however, sully these pages with the consideration of the foulest of the rascalities to which horse-racing has led. Simply to show those who bet on horse-races how many risks of loss they expose themselves to, I mention that some owners of horses have been known to bring about the defeat of their own

horse, on which the foolish betting public had wagered large sums, portions of which find their way into the pockets of the dishonest owners aforementioned. I may add that, according to an old proverb, there are more ways of killing a cat than by choking it with cream. A horse may be most effectually prevented from winning without any such vulgar devices as pulling, roping, and so forth. So also a horse, whose owner is honest, may be 'got at' after other fashions than have been noted yet, either in the police courts or in sporting novels.

Let us turn, however, from these unsavoury details, and consider briefly the objections which exist against gambling, even in the case of cash transactions so conducted that no unfair advantage is taken on either side.

The object of all gambling transactions is to win without the trouble of earning. I apprehend that nearly every one who wagers money on a horse race has, for some reason or other, faith in his own good fortune. It is a somewhat delicate question to determine how far such faith makes gambling unfair. For if, on the one hand, we must admit that a really lucky man could not fairly gamble against others not so lucky, yet, as it is absolutely certain in the scientific sense that no such thing as *luck which may be depended upon* exists, it is difficult to say how far faith in a non-existent quality can be held to make that fraudulent which would certainly be fraudulent did the quality exist. Possibly if a man, A, before laying a wager with another, B, were to

say, 'I have won nearly every bet I have made,' B might decline to encounter A in any wager. In the case of a man who had been so lucky as A, it is quite probable that, supposing a wager made with B and won by A, B would think he had been wronged if A afterwards told him of former successes. B might say, 'You should have told me that before I wagered with you; it is not fair to offer wagers where you know you have a better chance of winning than your opponents.' And though B would, strictly speaking, be altogether wrong, he would be reasoning correctly from his incorrect assumption, and A would be unable to contradict him.

If we were to assume that every man who wagered because he had faith in his own good luck was guilty of a moral though not of a logical or legal wrong, we should have to regard ninety-nine gamblers out of a hundred as wrong-doers. Let it suffice to point out that, whether believing in his luck or not, the gambler is blameworthy, since his desire is to obtain the property of another without giving an equivalent. The interchange of property is of advantage to society; because, if the interchange is a fair one, both parties to the transaction are gainers. Each exchanges something which is of less use to him for something which is of more use. This is equally the case whether there is a direct exchange of objects of value, or one of the parties to the exchange gives the other the benefit of his labour or of his skill acquired by labour. But in gambling, as where one man robs another, the case is otherwise. One person has lost what he can perhaps ill spare, while

the other has obtained what he has, strictly speaking, no right to, and what is almost certainly of less value to him than to the person who has lost it. Or, as Herbert Spencer concisely presents the case:—'Benefit received does not imply effort put forth, and the happiness of the winner involves the misery of the loser: this kind of action is therefore essentially anti-social; it sears the sympathies, cultivates a hard egoism, and so produces a general deterioration of character and conduct.'

BETTING ON RACES.

When I was travelling in Australasia, I saw a good deal of a class of men with whom, in this country, only betting men are likely to come much in contact —bookmakers, or men who make a profession of betting. What struck me most, perhaps, at first was that they regarded their business as a distinct profession. Just as a man would say in England, 'I am a lawyer or a doctor,' so these men would say that they were bookmakers. Yet, on consideration, I saw that there was nothing altogether novel in this. Others, whose business really is to gain money by making use of the weaknesses of their fellow-men, have not scrupled to call their employment a trade or a profession. Madame Rachel might have even raised her special occupation to the dignity of 'a mystery' on Shakespearean grounds ('Painting, sir, I have heard say is a mystery, and members of my occupation using painting, do prove my occupation a mystery'); and if aught of wrong in his employment could be made out to the satisfaction of a bookmaker, his answer might be Shakespearean also, 'Other sorts offend as well as we—ay, and better (qy. bettor) too.'

My own views about betting and bookmaking are regarded by many as unduly harsh, though I have admitted that the immorality which I find in betting has no existence with those who have not weighed the considerations on which a just opinion is based. I regard betting as essentially immoral so soon as its true nature is recognised. When a wager is made, and when after it has been lost and won its conditions are fulfilled, money has passed from one person to another without any 'work done' by which society is benefited. The feeling underlying the transaction has been greed of gain, however disguised as merely strong advocacy of some opinion—an opinion, perhaps, as to whether some horse will run a certain distance faster than another, whether certain dice will show a greater or less number of points, or the like. If here and there some few are to be found so strangely constituted mentally as really to take interest in having correct opinions on such matters, they are so few that they do not affect the general conclusion. They may bet to show they really think in such and such a way, and not to win money; but the great majority of betting men, professional (save the mark) or otherwise, want to win money, which is right enough, and to win money without working or doing some good for it, which is essentially immoral. That in a very large proportion of cases this negative immorality assumes a positive form—men trying to make unfair wagers (by betting with unfair knowledge of the real chances) —no one acquainted with the betting world, no one

who reads a sporting paper, no one even who reads the sporting columns of the daily papers, can fail to see. Why, if half the assurances of the various sporting prophets were trustworthy, betting, assisted by their instructions, would be as dishonourable as gambling with marked cards, as dishonest as picking pockets. Here is my 'Vaticinator,'[1] the betting man might say, who says that Roguery is almost sure to win the 'Beggar my Neighbour' stakes, but if he does not, that speedy mare, Rascality, will unquestionably win. Here are the bookmakers, who seem all quite as ready to lay the odds against Roguery and Rascality as against any of the other horses, to say nothing of my friends, Verdant and Flathead, who will freely back any of these latter. Now, if I back Roguery and Rascality with the bookmakers, and lay odds against the certain losers in the race, I shall certainly win all round. Of course, 'Vaticinator' is not the prophet he claims to be, but the betting man of our soliloquy supposes that he is; and so far as the morality of the course the latter follows is concerned the case is the same as though 'Vaticinator's' prophecies were gospel. There is not a particle of real distinction between what the bettor wants to do, and what a gambler, with cogged dice or marked cards, actually does. The more knowing a betting man claims to be, the easier it is to see that he wants and expects to take unfair advantage of other men. Either he

[1] I hope there is no turf prophet with this *nom-de-plume*. I know of none, or I would not use the name; but it may have been hit upon by some sporting man with a taste for polysyllables.

knows more than those he bets with about the real conditions of the race or contest on which they wager, or he does not. If he does, he wagers with them unfairly, and might as well pick their pockets. If he does not, but fancies he does, he is as dishonest in intention as he is in the former case in reality. If he does not, and knows he does not, he simply lies in claiming to know more than he does. In claiming to be knowing, he really claims to be dishonest and (which is not quite the same thing) dishonourable; and probably his claim is just.

To turn, however, to betting on horse-races as actually conducted.

There appears every day in the newspapers an account of the betting on the principal forthcoming races. The betting on such races as the Two Thousand Guineas, the Derby, and the Oaks, often begins more than a year before the races are run; and during the interval, the odds laid against the different horses engaged in them vary repeatedly, in accordance with the reported progress of the animals in their training, or with what is learned respecting the intentions of their owners. Many who do not bet themselves find an interest in watching the varying fortunes of the horses which are held by the initiated to be leading favourites, or to fall into the second rank, or merely to have an outside chance of success. It is amusing to notice, too, how frequently the final state of the odds is falsified by the event; how some 'rank outsider' will

run into the first place, while the leading favourites are not even 'placed.'

It is in reality a simple matter to understand the betting on races (or contests of any kind), yet it is astonishing how seldom those who do not actually bet upon races have any inkling of the meaning of those mysterious columns which indicate the opinion of the betting world respecting the probable results of approaching contests, equine or otherwise.

Let us take a few simple cases of 'odds,' to begin with; and, having mastered the elements of our subject, proceed to see how cases of greater complexity are to be dealt with.

Suppose the newspapers inform us that the betting is 2 to 1 against a certain horse for such and such a race, what inference are we to deduce? To learn this, let us conceive a case in which the *true* odds against a certain event are as 2 to 1. Suppose there are three balls in a bag, one being white, the others black. Then, if we draw a ball at random, it is clear that we are twice as likely to draw a black as to draw a white ball. This is technically expressed by saying that the odds are 2 to 1 *against* drawing a white ball; or 2 to 1 *on* (that is, in favour of) drawing a black ball. This being understood, it follows that, when the odds are said to be 2 to 1 against a certain horse, we are to infer that, in the opinion of those who have studied the performance of the horse, and compared it with that of the other horses engaged in the race, his chance of

winning is equivalent to the chance of drawing one particular ball out of a bag of three balls.

Observe how this result is obtained: the odds are 2 to 1, and the chance of the horse is as that of drawing one ball out of a bag of three—three being the sum of the two numbers 2 and 1. This is the method followed in all such cases. Thus, if the odds against a horse are 7 to 1, we infer that the *cognoscenti* consider his chance equal to that of drawing one particular ball out of a bag of *eight*.

A similar treatment applies when the odds are not given as so many to *one*. Thus, if the odds against a horse are as 5 to 2, we infer that the horse's chance is equal to that of drawing a white ball out of a bag containing five black and two white balls—or seven in all.

We must notice also that the number of balls may be increased to any extent, provided the proportion between the total number and the number of a specified colour remains unchanged. Thus, if the odds are 5 to 1 against a horse, his chance is assumed to be equivalent to that of drawing *one* white ball out of a bag containing six balls, only one of which is white; *or* to that of drawing a white ball out of a bag containing sixty balls, of which ten are white—and so on. This is a very important principle, as we shall now see.

Suppose there are two horses (amongst others) engaged in a race, and that the odds are 2 to 1 against one, and 1 to 1 against the other—what are the odds that one of the two horses will win the race? This

case will doubtless remind my readers of an amusing sketch by Leech, called—if I remember rightly—'Signs of the Commission.' Three or four undergraduates are at a 'wine,' discussing matters equine. One propounds to his neighbour the following question:—'I say, Charley, if the odds are 2 to 1 against *Rataplan*, and 4 to 1 against *Quick March*, what's the betting about the pair?'—'Don't know, I'm sure,' replies Charley, 'but I'll give you 6 to 1 against them.' The absurdity of the reply is, of course, very obvious; we see at once that the odds cannot be heavier against a pair of horses than against either singly. Still, there are many who would not find it easy to give a correct reply to the question. What has been said above, however, will enable us at once to determine the just odds in this or any similar case. Thus—the odds against one horse being 2 to 1, his chance of winning is equal to that of drawing one white ball out of a bag of *three*, one only of which is white. In like manner, the chance of the second horse is equal to that of drawing one white ball out of a bag of *five*, one only of which is white. Now we have to find a number which is a multiple of both the numbers three and five. Fifteen is such a number. The chance of the first horse, modified according to the principle explained above, is equal to that of drawing a white ball out of a bag of fifteen of which *five* are white. In like manner, the chance of the second is equal to that of drawing a white ball out of a bag of fifteen of which *three* are white. Therefore the chance that *one of the two* will win is equal to that

of drawing a white ball out of a bag of fifteen balls, of which *eight* (*five* added to *three*) are white. There remain *seven* black balls, and therefore the odds are 8 to 7 *on* the pair.

To impress the method of treating such cases on the mind of the reader, let us take the betting about three horses—say 3 to 1, 7 to 2, and 9 to 1 *against* the three horses respectively. Then their respective chances are equal to the chance of drawing (1) one white ball out of *four*, one only of which is white; (2) a white ball out of *nine*, of which two only are white; and (3) one white ball out of *ten*, one only of which is white. The least number which contains four, nine, and ten is 180; and the above chances, modified according to the principle explained above, become equal to the chance of drawing a white ball out of a bag containing 180 balls, when 45, 40, and 18 (respectively) are white. Therefore, the chance that one of the three will win is equal to that of drawing a white ball out of a bag containing 180 balls, of which 103 (the sum of 45, 40, and 18) are white. Therefore, the odds are 103 to 77 *on* the three.

One does not hear in practice of such odds as 103 to 77. But betting-men (whether or not they apply just principles of computation to such questions is unknown to me) manage to run very near the truth. For instance, in such a case as the above, the odds on the three would probably be given as 4 to 3—that is, instead of 103 to 77 (or 412 to 308), the published odds would be equivalent to 412 to 309.

And here a certain nicety in betting has to be mentioned. In running the eye down the list of odds, one will often meet such expressions as 10 to 1 against such a horse *offered*, or 10 to 1 *wanted*. Now, the odds of 10 to 1 *taken* may be understood to imply that the horse's chance is equivalent to that of drawing a certain ball out of a bag of eleven. But if the odds are offered and not taken, we cannot infer this. The offering of the odds implies that the horse's chance is *not better* than that above mentioned, but the fact that they are not taken implies that the horse's chance is *not so good*. If no higher odds are offered against the horse, we may infer that his chance is *very little worse* than that mentioned above. Similarly, if the odds of 10 to 1 are *asked for*, we infer that the horse's chance is *not worse* than that of drawing one ball out of eleven; if the odds are not obtained, we infer that his chance is *better*; and if no lower odds are asked for, we infer that his chance is *very little better*.

Thus, there might be *three* horses (A, B, and C) against whom the nominal odds were 10 to 1, and yet these horses might not be equally good favourites, because the odds might not be taken, or might be asked for in vain. We might accordingly find three such horses arranged thus:

	Odds.
A . . .	10 to 1 (wanted).
B . . .	10 to 1 (taken).
C . . .	10 to 1 (offered).

Or these different stages might mark the upward or

downward progress of the same horse in the betting. In fact, there are yet more delicate gradations, marked by such expressions respecting certain odds, as—*offered freely*, *offered*, *offered and taken* (meaning that some offers only have been accepted), *taken*, *taken and wanted*, *wanted*, and so on.

As an illustration of some of the principles I have been considering, let us take from the day's papers[1] the state of the odds respecting the 'Two Thousand Guineas.' It is presented in the following form:

TWO THOUSAND GUINEAS.

7 to 2 against *Rosicrucian* (off.).
6 to 1 against *Pace* (off.; 7 to 1 w.).
10 to 1 against *Green Sleeve* (off.).
100 to 7 against *Blue Gown* (off.).
180 to 80 against Sir J. Hawley's lot (t.).

This table is interpreted thus: bettors are willing to lay the same odds against *Rosicrucian* as would be the true mathematical odds against drawing a white ball out of a bag containing two white and seven black balls; but no one is willing to back the horse at this rate. On the other hand, higher odds are not offered against him. Hence it is presumable that his chance is but slightly less than that above indicated. Again, bettors are willing to lay the same odds against *Pace* as might fairly be laid against drawing one white ball out of a bag of seven, one only of which is white; but backers of the horse consider that they ought to get

[1] This was written early in March 1868.

the same odds as might be fairly laid against drawing the white ball when an additional black ball had been put into the bag. As respects *Green Sleeve* and *Blue Gown*, bettors are willing to lay the odds which there would be, respectively, against drawing a white ball out of a bag containing—(1) eleven balls, one only of which is white, and (2) one hundred and seven balls, seven only of which are white. Now, the three horses, *Rosicrucian*, *Green Sleeve*, and *Blue Gown*, all belong to Sir Joseph Hawley, so that the odds about the three are referred to in the last statement of the list just given. And since none of the offers against the three horses have been taken, we may expect the odds actually taken about 'Sir Joseph Hawley's lot' to be more favourable than those obtained by summing up the three former in the manner we have already examined. It will be found that the resulting odds (offered) against Sir J. Hawley's lot—estimated in this way—should be, as nearly as possible, 132 to 80. We find, however, that the odds *taken* are 180 to 80. Hence, we learn that the offers against some or all of the three horses are considerably short of what backers require; or else that some person has been induced to offer far heavier odds against Sir J. Hawley's lot than are justified by the fair odds against his horses, severally.

I have heard it asked why a horse is said to be a favourite, though the odds may be against him. This is very easily explained. Let us take as an illustration the case of a race in which four horses are engaged to

run. If all these horses had an equal chance of winning, it is very clear that the case would correspond to that of a bag containing four balls of different colours; since, in this case, we should have an equal chance of drawing a ball of any assigned colour. Now, the odds against drawing a particular ball would clearly be 3 to 1. This, then, should be the betting against each of the three horses. If any one of the horses has less odds offered against him, he is a *favourite*. There may be more than one of the four horses thus distinguished; and, in that case, the horse against which the least odds are offered is *the first favourite*. Let us suppose there are two favourites, and that the odds against the leading favourite are 3 to 2, those against the other 2 to 1, and those against the best non-favourite 4 to 1; and let us compare the chances of the four horses. I have not named any odds against the fourth, because, if the odds against all the horses but one are given, the just odds against that one are determinable, as we shall see immediately. The chance of the leading favourite corresponds to the chance of drawing a ball out of a bag in which are three black and two white balls, *five* in all; that of the next to the chance of drawing a ball out of a bag in which are two black and one white ball, *three* in all; that of the third, to the chance of drawing a ball out of a bag in which are four black balls and one white one, *five* in all. We take, then, the least number containing both five and three—that is, *fifteen*; and then the number of white balls corresponding to the chances of the three horses are respectively six, five, and three,

or fourteen in all; leaving only *one* to represent the chance of the fourth horse (against which the odds are therefore 14 to 1). Hence the chances of the four horses are respectively as the numbers *six*, *five*, *three*, and *one*.

I have spoken above of the published odds. The statements made in the daily papers commonly refer to wagers actually made, and therefore the uninitiated might suppose that everyone who tried would be able to obtain the same odds. This is not the case. The wagers which are laid between practised betting-men afford very little indication of the prices which would be forced (so to speak) upon an inexperienced bettor. Bookmakers—that is, men who make a series of bets upon several or all of the horses engaged in a race—naturally seek to give less favourable terms than the known chances of the different horses engaged would suffice to warrant. As they cannot offer such terms to the initiated, they offer them—and in general successfully—to the inexperienced.

It is often said that a man may so lay his wagers about a race as to make sure of gaining money whichever horse wins the race. This is not strictly the case. It is of course possible to make sure of winning if the bettor can only get persons to lay or take the *odds he requires to the amount he requires*. But this is precisely the problem which would remain insoluble if all bettors were equally experienced.

Suppose, for instance, that there are three horses engaged in a race with equal chances of success. It is

readily shown that the odds are 2 to 1 against each. But if a bettor can get a person to take even betting against the first horse (A), a second person to do the like about the second horse (B), and a third to do the like about the third horse (C), and if all these bets are made to the same amount—say 1,000*l.*—then, inasmuch as only one horse can win, the bettor loses 1,000*l.* on that horse (say A), and gains the same sum on each of the two horses B and C. Thus, on the whole, he gains 1,000*l.*, the sum laid out against each horse.

If the layer of the odds had laid the true odds to the same amount on each horse, he would neither have gained nor lost. Suppose, for instance, that he laid 1,000*l.* to 500*l.* against each horse, and A won; then he would have to pay 1,000*l.* to the backer of A, and to receive 500*l.* from each of the backers of B and C. In like manner, a person who had backed each horse to the same extent would neither lose nor gain by the event. Nor would a backer or layer who had wagered *different* sums *necessarily* gain or lose by the race; he would gain or lose *according to the event.* This will at once be seen, on trial.

Let us next take the case of horses with unequal prospects of success—for instance, take the case of the four horses considered above, against which the odds were respectively 3 to 2, 2 to 1, 4 to 1, and 14 to 1. Here, suppose the same sum laid against each, and for convenience let this sum be 84*l.* (because 84 contains the numbers 3, 2, 4, and 14). The layer of the odds

wagers 84*l.* to 56*l.* against the leading favourite, 84*l.* to 42*l.* against the second horse, 84*l.* to 21*l.* against the third, and 84*l.* to 6*l.* against the fourth. Whichever horse wins, the layer has to pay 84*l.*; but if the favourite wins, he receives only 42*l.* on one horse, 21*l.* on another, and 6*l.* on the third—that is 69*l.* in all, so that he loses 15*l.*; if the second horse wins, he has to receive 56*l.*, 21*l.*, and 6*l.*—or 83*l.* in all, so that he loses 1*l.*; if the third horse wins, he receives 56*l.*, 42*l.*, and 6*l.*—or 104*l.* in all, and thus gains 20*l.*; and lastly, if the fourth horse wins, he has to receive 56*l.*, 42*l.*, and 21*l.*—or 119*l.* in all, so that he gains 35*l.* He clearly risks much less than he has a chance (however small) of gaining. It is also clear that in all such cases the worst event for the layer of the odds is that the first favourite should win. Accordingly, as professional bookmakers are nearly always layers of odds, one often finds the success of a favourite spoken of in the papers as a 'great blow for the bookmakers,' while the success of a rank outsider will be described as a 'misfortune to backers.'

But there is another circumstance which tends to make the success of a favourite a blow to layers of the odds and *vice versâ*. In the case we have supposed, the money actually pending about the four horses (that is, the sum of the amounts laid *for* and *against* them) was 140*l.* as respects the favourite, 126*l.* as respects the second, 105*l.* as respects the third, and 90*l.* as respects the fourth. But, as a matter of fact, the amounts pending about the favourites bear always

a much greater proportion than the above to the amounts pending about outsiders. It is easy to see the effect of this. Suppose, for instance, that instead of the sums 84*l.* to 56*l.*, 84*l.* to 42*l.*, 84*l.* to 21*l.*, and 84*l.* to 6*l.*, a bookmaker had laid 8,400*l.* to 5,600*l.*, 840*l.* to 420*l.*, 84*l.* to 21*l.*, and 14*l.* to 1*l.*, respectively—then it will easily be seen that he will lose 7,958*l.* by the success of the favourite; whereas he would gain 4,782*l.* by the success of the second horse, 5,937*l.* by that of the third, and 6,027*l.* by that of the fourth. I have taken this as an extreme case; as a general rule, there is not so great a disparity as has been here assumed between the sums pending on favourites and outsiders.

Finally, it may be asked whether, in the case of horses having unequal chances, it is possible that wagers can be so proportioned (just odds being given and taken) that, as in the former case, a person backing or laying against all the four shall neither gain nor lose. It is so. All that is necessary is, that the sum actually pending about each horse shall be the same. Thus, in the preceding case, if the wagers 9*l.* to 6*l.*, 10*l.* to 5*l.*, 12*l.* to 3*l.*, and 14*l.* to 1*l.*, are either laid or taken by the same person, he will neither gain nor lose by the event, whatever it may be. And therefore if unfair odds are laid or taken about all the horses, in such a manner that the amounts pending on the several horses are equal (or nearly so), the unfair bettor must win by the result. Say, for instance, that instead of the above odds, he lays 8*l.* to 6*l.*, 9*l.* to 5*l.*, 11*l.* to 3*l.*, and 13*l.* to

1*l.* against the four horses respectively; it will be found that he *must* win 1*l.* Or if he *takes* the odds 18*l.* to 11*l.*, 20*l.* to 9*l.*, 24*l.* to 5*l.*, and 28*l.* to 1*l.* (the just odds being 18*l.* to 12*l.*, 20*l.* to 10*l.*, 24*l.* to 6*l.*, and 28*l.* to 2*l.* respectively), he will win 1*l.* by the race. So that, by giving or taking such odds to a sufficiently large amount, a bettor would be certain of pocketing a considerable sum, whatever the event of a given race might be.

It is by no means necessary that the system I have described above should be carried out in a precise and formal manner. If you have a tolerably large capital, or if, in case of failure, you have courage (greatly daring) to run away, you may leave a little to chance on every race, and then, if chance favours you, your gains will be proportionately greater.

But for supreme success on the turf, wider measures must be adopted, which may now be sketched in outline. The system is exceedingly simple—and it will be found that when the method of the great bookmakers is analysed a little, there underlies it the fundamental idea of the system—yet probably not one among them knows anything about it in detail, though he may thoroughly well understand that his method leaves very little to chance.

Viewing the matter then from the point of view of those who make a business of betting on horses, and regard themselves as in the profession, here are the rules for a success:

First, the bookmaker must always lay odds against

horses, never back them. This is not essential to the system regarded in its scientific aspect; but in practice, as will presently appear, it makes it easier to apply it.

Next, he lays against nearly every horse in a race as early as possible, when the odds are longest. If he lays against a few which are certain not to run, so much the better for him; that is so much clear gain to start with. He should proportion his wagers so that the sum of what he lays against a horse, and what he is backed for, may amount to about the same for each horse. The precise system requires that it should be exactly the same, but the bookmaker often improves upon that by taking advantage, in special cases, of his own knowledge of a horse's chance and his opponent's inexperience. In every case he lays odds a point or two short of the legitimate odds against a horse. Suppose for a moment that the odds are ten to one against the horse, then it is always easy to find folk who rather fancy the horse, and think the odds are not eight to one, or even six to one, against him; he selects such persons for his wagers about that horse. He conveys carefully the idea that he thinks the horse's chance underrated at eight, or even nine to one; but, as a favour, he will make the odds nine to one. Of course, he has no occasion to search about for those who favour any given horse. Every greenhorn has a fancy for some horse, and is willing to take something short of the current odds for the privilege of backing him. The bookmaker can therefore fill in his book *pro re natâ*,

until at least he has made up sufficient amounts for most of the horses engaged, when, of course, he gives more special attention to those whose leaf in his book is as yet incomplete.

Now, let us take an illustrative case to see how this system works:

Suppose there are nine horses in the race, to wit:—A, B, C, D, E, F, G, H, and K. Let the odds be—

3 to 1 against A	11 to 1 against F
5 to 1 „ B	11 to 1 „ G
7 to 1 „ C	19 to 1 „ H
9 to 1 „ D	23 to 1 „ K
9 to 1 „ E	

(It should be noted that when these odds are reduced to chances, becoming respectively

$$\frac{1}{4},\ \frac{1}{6},\ \frac{1}{8},\ \frac{1}{10},\ \frac{1}{10},\ \frac{1}{12},\ \frac{1}{12},\ \frac{1}{20},\ \frac{1}{24},$$

their sum should be unity or very near it. It does not matter at all—except to backers—if the sum is greater than unity, as it generally is; but if it should be less than unity, the exact application of the system would involve loss to the bookmaker and gain to backers, which is not the bookmaker's object.)

Suppose now the wagers on each horse amount to 1,000*l.* (or for convenience, and to avoid fractions, say 1,200*l.*), if the race is important, and bets much in request; though the system, in its beautiful adaptability, may be applied to shillings quite as well as to pounds.

Apart from the extra points which the bookmaker allows himself, he may lay, in all, about—

£900 to £300	against	A
£1,000 to £200	„	B
£1,050 to £150	„	C
£1,080 to £120	„	D
£1,080 to £120	„	E
£1,100 to £100	„	F
£1,100 to £100	„	G
£1,140 to £60	„	H
£1,150 to £50	„	K

But he reasons (with intending backers) that 'the race is a moral certainty for A, and that it is giving away money' to lay more than (in all) 800*l.* to 300*l.* Again, 'B is a much better horse than people think, so that 900*l.* to 200*l.* is quite long enough odds against him;' as for C, 'no wonder backers stand by him at the odds;' for his part the bookmaker 'thinks him better than B; and see what Augur says of him!' and so forth, wherefore he cannot find it in his conscience to lay more than 950*l.* to 150*l.* (in all) against him. (It gets easier as the non-favourites are reached to get the odds shortened.) So he deals with each, cutting off about 100*l.* (let us say) from the amount he ought to lay against them severally; but with the horses low in favour, he can easily cut off more, and the system not only does not forbid this but encourages it. Say, however, only 100*l.*, and then his book is complete.

The bookmaker can now watch the race with thorough enjoyment. The pleasure of the backers of

the favourites is a good deal impaired by anxiety, and though backers of non-favourites have less to lose, they have more to gain, and less chance of gaining it: so they too are anxious. But the bookmaker can watch the race with perfect calmness.

For, let the race go as it may, he must clear 100*l.* If A win, the bookmaker willingly pays A's backers 800*l.*, receiving 200*l.* from the backers of B, 150*l.* from those of C, and so on—in all, 900*l.* If B win, the bookmaker pays B's backers 900*l.*, and receives from the backers of A, C, D, &c., 1,000*l.*; and so on, whichever horse may win. There is not, as a rule, any fear about being paid; these are debts of honour, and to be paid before all sordid trade debts—nay, so sacred are these debts, that many of the bookmaker's clients would deem it better to break open a till, or to embezzle a round sum from an employer, than to leave them unpaid. So he is under no anxiety.

Thus does the bookmaker make a steady income out of his victims, who go not only complacently to their fate, but even with a look of wisdom as if they were rather cleverly taking advantage of the proffered gifts of fortune.

It is easier to tell how they lose than to show how the bookmaker gains. They adopt *the other and simpler part* of the bookmaker's system. *He* always lays the odds a little short: they always take them so. They back the favourite boldly, but they do not fail to take fancies for non-favourites, and to back their fancies boldly too. It would be absurd to haggle about odds

in the case of a horse which is morally sure to win, or to insist on ten to one when sure the odds are not seven to one against a horse. When the simpleton wins he assures himself he is 'in the vein,' and goes on betting; if he loses, he assures himself 'the luck must change,' and goes on betting. By continuing patiently on this course, it will be odd if he do not learn before long —how it is that the bookmakers make so much money.

Of course I have given here but a mild account of the way in which men who bet on horses make money. They have been known to go a great deal farther. Some will willingly take the odds against a horse after they knew certainly that the horse would not run. Others, a shade more advanced, have been known to bribe a jockey to 'hold' or 'rope' a horse, or a stableman to poison or even stupefy him. Others, ay, even 'noble' owners, have been known to work the market in ways fully as flagitious.

Let me, in conclusion, quote two short passages, one from a letter by Charles Dickens, the other from a speech by Lord Chief Justice Cockburn. The first seems to relate to the successful bookmaker: —'I look at the back of his bad head repeated in long lines on the racecourse, and in the betting-stand, and outside the betting-rooms, and I vow to God I can see nothing in it but cruelty, covetousness, calculation, insensibility, and low wickedness. . . . If a boy with any good in him, but with a dawning propensity to sporting and betting, were but brought here soon enough, it would cure him.' The other passage

applies to the bookmaker and his victim alike:—'The pernicious and fatal habit' of betting 'is so demoralising and degrading, that, like some foul leprosy, it will eat away the conscience until a man comes to think that it is his duty to himself to "do his neighbour as his neighbour would do" him.'

LOTTERIES.

Long experience has shown that men possessed with the gambling spirit (ninety out of a hundred if the truth were known) are not to be deterred from venturing small sums in order to win large fortunes, even by the clearest evidence that the price they have to pay is an unfair one. The Government lotteries in this country early put this matter to the test. Having decided on a certain set of money prizes and a certain number of tickets, the Government did not offer the tickets to the public for more than they were worth, but for what they would fetch. They seldom failed to obtain from contractors at least 16*l.* for a ticket mathematically worth 10*l.* And the contractors not only showed by offering these sums their faith in human credulity, but practically proved the truth of their faith by disposing of their tickets for 5*l.* or 6*l.* more than they had paid Government for them. Thus the Government occupied a very favourable position. For every million they offered in prizes they received more than 1,600,000*l.*; yet they asked no one to pay an unfair price. They left the contractors to do that, who were not only willing, but anxious to

undertake the task of shearing the public. Nor were the public less ready to be plundered than the contractors were to plunder them. Government had to protect the public, or rather tried to protect them, from the contractors, not by putting a limit to the price which contractors might obtain for tickets, but by endeavouring to prevent men of small means from buying tickets in shares of less than a certain value. Of course, the laws made for this purpose were readily and systematically broken. The smallest sums were risked, and the only effect of the laws against such purchases was that higher prices had to be paid to cover the risk of detection. We learn that 'all the efforts of the police were ineffectual for the suppression of these illegal proceedings, and for many years a great and growing repugnance was manifested in Parliament to this method of raising any part of the public revenue. At length, in 1823, the last Act that was sanctioned by Parliament for the sale of lottery-tickets contained provisions for putting down all private lotteries, and for rendering illegal the sale in this kingdom of all tickets or shares of tickets in any foreign lottery—which latter provision is to this day extensively evaded.' This was written forty years ago, but might have been written to-day.

The simplest, and in many respects the best, form of lottery is that in which a number of articles are taken as prizes, their retail prices added together, and the total divided into some large number of parts, the same number of tickets being issued at the price thus

indicated. Suppose, for instance, the prizes amount in value to 200*l.*, then a thousand tickets might be sold at 4*s.* each, or 4,000 at 1*s.* each, or a larger number at a correspondingly reduced price. In such a case the lottery is strictly fair, supposing the prizes in good saleable condition. The person who arranges the lottery gains neither more nor less than he would if he sold the articles separately. There may be a slight expense in arranging the lottery, but this is fully compensated by the quickness of the sale. The arrangement, I say, is fair; but I do not say it is desirable, or even that it should be permissible. Advantage is taken of the love of gambling, innate in most men, to make a quick sale of goods which otherwise might have lain long on hand. Encouragement is given to a tendency which is inherently objectionable if not absolutely vicious. And so far as the convenience is concerned of those who collectively buy (in fact) the prizes, it manifestly cannot be so well suited as though those only had bought who really wanted the articles, each taking the special article he required. Those who buy tickets want to get more than their money's worth. Some of them, if not all, are believers in their own good luck, and expect to get more than they pay for. They are willing to get, in this way, something which very likely they do not want, something therefore which will be worth less to them in reality than the price for which it is justly enough valued in the list of prizes.

Unfortunately those who arrange lotteries of this sort for mere trade purposes (they are not now allowed

in this country, but abroad they are common enough, and English people are invited to take part in these foreign swindles) are not careful to estimate the price of each article justly. They put a fancy price on good articles, a full price on damaged articles, and throw in an extra sum for no articles at all. Many of them are not at all particular, if the sale of tickets is quick, about throwing in a few hundred more tickets than they had originally provided for, without in the least considering it necessary to add correspondingly to the list of prizes.

But this is not all. How much those who arrange such lotteries really wrong the purchasers of tickets cannot be known. But we can learn how ready the ticket-buyers are to be wronged, when we note what they will allow. It seems absurd enough that they should let the manager of a lottery act entirely without check or control as to the number of tickets or the plan according to which these are drawn. But at least when a day is appointed for the drawing, and the prizes are publicly exhibited in the first instance, and as publicly distributed eventually, the ticket-buyers know that the lottery has been in some degree *bonâ fide*. What, however, can we think of those who will pay for the right of drawing a ticket from a 'wheel of fortune,' without having the least means of determining what is marked on any of the tickets, or whether a single ticket is marked for a prize worth more than the price paid for a chance, or even worth as much? Yet nothing is more common where such wheels are allowed, and nothing was more common when they were allowed here, than for a shop-

man to offer for a definite sum, which frequenters of the shop would readily pay, the chance of drawing a prize-ticket out of a wheel of fortune, though he merely assured them, without a particle of proof, that some of the tickets would give them prizes worth many times the price they paid. Even when there were such tickets, again, and someone had secured a prize (though the chances were that the prize-drawer was connected with the business), people who had seen this would buy chances as though the removal of one good prize-ticket had made no difference whatever in the value of a chance. They would actually be encouraged to buy chances by the very circumstance which should have deterred them. For if a good prize is drawn in such a case, the chances are that no good prize is left.

Although lotteries of this sort are no longer allowed by law, yet are they still to some degree countenanced in connection with charity and the fine arts. Now, setting aside lotteries connected with the fine arts as singularly mixed in character—though it must not for a moment be supposed that I regard a taste for gambling with a love of the beautiful as forming an agreeable mixture—I note that in lotteries started for charitable purposes there is usually no thought of gain on the part of those who originate the scheme. That is, they have no wish to gain money for themselves, though they may be very anxious to gain money for the special purpose they have in view. This wish may be, and indeed commonly proves to be, inconsistent with strict fairness towards the buyers of tickets. But as

these are supposed to be also possessed with the same desire to advance a charitable purpose which actuates the promoters of the scheme, it is not thought unfair to sell them their tickets rather dearly, or to increase the number of tickets beyond what the true value of the prizes would in strict justice permit. It is, however, to be noted that the assumption by which such procedure is supposed to be justified is far from being always accurate. It is certain that a large proportion of those who buy tickets in charitable lotteries take no interest whatever in the object for which such lotteries are started. If lotteries were generally allowed, and therefore fairer lotteries could be formed than the charitable ones—which are as unfair in reality as the dealings of lady stall-keepers at fancy bazaars—the sale of tickets at charitable lotteries would be greatly reduced. It is only because those who are possessed by the gambling spirit can join no other lotteries that they join those started for charitable purposes. The managers of these lotteries know this very well, though they may not be ready to admit very publicly that they do. If pressed on the subject, they speak of spoiling the Egyptians, of the end justifying the means, and so forth. But, as a matter of fact, it remains true that these well-intentioned folk, often most devout and religious persons, do, in the pursuit of money for charitable purposes, pander to the selfishness and greed of the true gambler, encourage the growth of similar evil qualities in members of their own community, and set an evil example, moreover, by systematically breaking the law

of the country. It would be harsh, perhaps, to speak strongly against persons whose intentions are excellent, and who are in many cases utterly free from selfish aims; but they cannot be acquitted from a charge of extreme folly, nor can it be denied that, be their purpose what it may, their deeds are evil in fact and evil in their consequences. It might be difficult to determine whether the good worked by the total sum gained from one of these charitable lotteries was a fair equivalent for the mischief wrought in getting it. But this total is not all gained by choosing an illegal method of getting the sum required. The actual gain is only some slight saving of trouble on the part of the promoters of the charitable scheme, and a further slight gain to the pockets of the special community in which the charity is or should be promoted. And it is certain that these slight gains by no means justify the use of an illegal and most mischievous way of obtaining money. It would be difficult to find any justification for the system, once the immorality of gambling is admitted, which might not equally well be urged for a scheme by which the proceeds (say) of one week's run of a common gaming-table should be devoted to the relief of the sick poor of some religious community. Nay, if charitable ends can at all justify immoral means, one might go further still, and allow money to be obtained for such purposes by the encouragement of still more objectionable vices. We might in fact recognise quite a new meaning in the saying that 'Charity covers a multitude of sins.'

I have said that a lottery in which all the prizes were goods such as might be sold, retail, at prices amounting to the total cost of all the tickets sold, would be strictly fair. I do not know whether a lottery ever has been undertaken in that way. But certainly it seems conceivable that such a thing might have happened; and in that case, despite the objections which, as we have shown, exist against such an arrangement, there would have been a perfectly fair lottery. Adam Smith, in his 'Wealth of Nations,' seems to have omitted the consideration of lotteries of this kind, when he said that 'the world neither ever saw, nor ever will see, a perfectly fair lottery, or one in which the whole gain compensated the whole loss; because the undertaker could gain nothing by it.' Indeed, it has certainly happened in several cases that there have been lotteries in which the total price of the tickets fell short of the total value of the prizes—these being presents made for a charitable purpose, and the tickets purposely sold at very low prices. It is well known, too, that in ancient Rome, where lotteries are said to have been invented, chances in lotteries were often, if not always, distributed gratuitously.

But assuredly Adam Smith is justified in his remark if it be regarded as relating solely to lotteries in which the prizes have been sums of money, and gain has been the sole object of the promoters. 'In the State lotteries,' as he justly says, 'the tickets are really not worth the price which is paid by the original subscribers,' though from his sequent remarks it appears that he had

very imperfect information respecting some of the more monstrous cases of robbery (no other word meets the case) by promoters of some of these State swindles.

The first idea in State lotteries seems to have been to adopt the simple arrangement by which a certain sum is paid for each of a given number of tickets, the series of prizes provided being less in total value than the sum thus obtained.

It was soon found, however, as I have already pointed out, that people are easily gulled in matters of chance, so that the State could safely assume a very disinterested attitude. Having provided prizes of definite value, and arranged the number of tickets, it simply offered these for sale to contractors. The profit to the State consisted in the excess of the sum which the contractors willingly offered above the just value (usually 10*l.*) of each ticket. This sum varied with circumstances, but generally was about 6*l.* or 7*l.* per ticket beyond the proper price. That is, the contractors paid about 16*l.* or 17*l.* for tickets really worth 10*l.* They were allowed to divide the tickets into shares—halves, quarters, eighths, and sixteenths. When a contractor sold a full ticket he usually got from 21*l.* to 22*l.* for it; but when he sold a ticket in shares his gain per ticket was considerably greater. The object in limiting the subdivision to one-sixteenth was to prevent labouring men from risking their earnings.

It is hardly necessary to say, however, that the provision was constantly and easily evaded, or that

the means used for evading the limitation only aggravated the evil. At illegal offices, commonly known as 'little goes,' any sum, however small, could be risked, and to cover the chance of detection and punishment these offices required greater profits than the legal lottery offices. Precisely as attempts to prevent usury caused the necessitous borrowers of money to be mulcted even more severely than they would otherwise have been, so the attempt to protect the poor from falling into gambling ways resulted only in driving them to gamble against more ruinous odds.

The record of national lotteries in England ranges over two centuries and a half. It forms an interesting, though little studied, chapter in the history of the nation, and throws curious light on the follies and weaknesses of human nature.

The earliest English lottery on record is that of the year 1569, when 40,000 chances were sold at 10*s.* each, the prizes being articles of plate, and the profit used in the repair of certain harbours. The gambling spirit seems to have developed greatly during the next century; for, early in the reign of Queen Anne, it was found necessary to suppress private lotteries 'as public nuisances,' a description far better applicable (in more senses than one) to public lotteries. 'In the early period of the history of the National Debt,' says a writer (De Morgan, I believe) in the 'Penny Cyclopædia,' 'it was usual to pay the prizes in the State lotteries in the form of terminable annuities. In 1694 a loan of a million was raised by the sale of lottery-tickets at 10*l.* per ticket,

the prizes in which were funded at the rate of 14 per cent. for sixteen years certain. In 1746 a loan of three millions was raised on 4 per cent. annuities, and a lottery of 50,000 tickets of 10*l.* each; and in the following year one million was raised by the sale of 100,000 tickets, the prizes in which were funded in perpetual annuities at the rate of 4 per cent. per annum. Probably the last occasion on which the taste for gambling was thus made use of occurred in 1780, when every subscriber of 1,000*l.* towards a loan of twelve millions, at 4 per cent., received a bonus of four lottery-tickets, the intrinsic value of each of which was 10*l.*' About this time the spirit of gambling had been still more remarkably developed than in Anne's reign, despite the laws passed to suppress private lotteries. In 1778 an Act was passed by which every person keeping a lottery-office was obliged to take out a yearly license costing 50*l.* This measure reduced the number of such offices from 400 to 51. In France the demoralisation of the people resulting from the immorality of the Government in encouraging by lotteries the gambling spirit, was greater even than in England.

The fairest system for such lotteries as we have hitherto considered was that adopted in the Hamburg lotteries. Here, the whole money for which tickets were sold was distributed in the form of prizes, except a deduction of 10 per cent. made from the amount of each prize at the time of payment.

Before pausing to consider the grossly unfair systems which have been, and still are, adopted in certain foreign

lotteries, it may be well to notice that the immorality of lotteries was not recognised a century ago so clearly as it is now; and therefore, in effect, those who arranged them were not so blameworthy as men are who, in our own time, arrange lotteries, whether openly or surreptitiously. Even so late as half a century ago an American lawyer, of high character, was not ashamed openly to defend lotteries in these terms. 'I am no friend,' he said, 'to lotteries, but I cannot admit that they are *per se* criminal or immoral when authorised by law. If they were nuisances, it was in the manner in which they were managed. In England, if not in France' (how strange this sounds), 'there were lotteries annually instituted by Government, and it was considered a fair way to reach the pockets of misers and persons disposed to dissipate their funds. The American Congress of 1776 instituted a national lottery, and perhaps no body of men ever surpassed them in intelligence and virtue.' De Morgan, remarking on this expression of opinion, says that it shows what a man of high character for integrity and knowledge thought of lotteries twenty years ago (he wrote in 1839). 'The opinions which he expressed were at that time,' continued De Morgan, 'shared, we venture to say, by a great number.'

The experience of those who arranged these earlier State lotteries showed that from men in general, especially the ignorant (forming the great bulk of the population who place such reliance on their luck), almost any price may be asked for the chance of making

a large fortune at one lucky stroke. Albeit, it was seen that the nature of the fraud practised should preferably be such that not one man in a thousand would be able to point out where the wrong really lay. Again, it was perceived that if the prizes in a lottery were reduced too greatly in number but increased in size, the smallness of the chance of winning one of the few prizes left would become too obvious. A system was required by which the number of prizes might seem unlimited and their possible value very great, while also there should be a possibility of the founders of the lottery not getting back all they ventured. So long as it was absolutely certain that, let the event be what it might, the managers of the lottery would gain, some might be deterred from risking their money by the simple statement of this fact. Moreover, under such conditions, it was always possible that at some time the wrath of losers (who would form a large part of the community if lottery operations were successful) might be roused in a dangerous way, unless it could be shown that the managers of public lotteries ran some chance, though it might be only a small chance, of losing, and even some chance of ruin as absolute as that which might befall individual gamblers.

It was to meet such difficulties as these that lottery systems like that sometimes called the Geneva system were invented. This system I propose now to describe, as illustrating these more speculative ventures, showing in particular how the buyers of chances were defrauded in the favourite methods of venturing.

In the Geneva lottery there are ninety numbers. At each drawing five are taken. The simplest venture is made on a single number. A sum is hazarded on a named number, and if this number is one of the five drawn, the speculator receives fifteen times the value of his stake. Such a venture is called a *simple drawing*. It is easy to see that in the long run the lottery-keeper must gain by this system. The chance that the number selected out of ninety will appear among five numbers drawn, is the same that a selected number out of eighteen would appear at a single drawing. It is one chance in eighteen. Now if a person bought a single ticket out of eighteen, each costing (say) 1*l.*, his fair prize if he drew the winning ticket should be 18*l.* This is what he would have to pay to buy up all the eighteen tickets (so making sure of the prize). The position of the speculator who buys one number at 1*l.* in the Geneva lottery, is precisely that of a purchaser of such a ticket, only that, instead of a prize being 18*l.* if he wins, it is only 15*l.* The lottery-keeper's position on a single venture is not precisely that of one who should have sold eighteen tickets at 1*l.* each, for a lottery having one prize only; for the latter would be certain to gain money if the prize were any sum short of 18*l.*, whereas the Geneva lottery-keeper will lose on a single venture, supposing the winning number is drawn, though the prize is 15*l.* instead of 18*l.* But in the long run the Geneva lottery-keeper is certain to win at these odds. He is in the position of a man who continually wagers odds of 14 to 1 against the occurrence of an event the

real odds against which are 17 to 1. Or his position may be compared to that of a player who takes seventeen chances out of eighteen at (say) their just value, 1*l.* each or 17*l.* in all, his opponent taking the remaining chance at its value, 1*l.*, but instead of the total stakes, 18*l.*, being left in the pool, the purchaser of the larger number abstracts 3*l.* from the pool at each venture.

That men can be found to agree to such an arrangement as this shows that their confidence in their own good fortune makes them willing to pay, for the chance of getting fifteen times their stake, what they ought to pay for the chance of getting eighteen times its value. The amount of which they are in reality defrauded at each venture is easily calculated. Suppose the speculator to venture 1*l.* Now the actual value of one chance in eighteen of any prize is one-eighteenth of that prize, which in this case should therefore be 18*l.* If, then, the prize really played for has but fifteen-eighteenths of its true value, or is in this case 15*l.*, the value of a single chance amounts only to one-eighteenth of 15*l.*, or to 16*s.* 8*d.* Thus at each venture of 1*l.* the speculator is cheated out of 3*s.* 4*d.*, or one-sixth of his stake.

This, however, is a mere trifle. In the old-fashioned English system of lotteries, the purchaser of a 10*l.* ticket often paid more than 20*l.*, so that he was defrauded by more than half his stake; and though less than half the robbery went into the hands of the contractor who actually sold the ticket, the rest of the robbery went to the State.

In other ventures, by the Geneva system, the

old-fashioned English system of robbery was far surpassed.

Instead of naming one number for a drawing (in which five numbers are taken) the speculator may say in what position among the five his number is to come. If he is successful, he receives seventy times his stake. This is, in effect, exactly the same as though but one number was drawn. The speculator has only one chance out of ninety instead of one chance out of five. He ought then, in strict justice, to receive ninety times his stake, if he wins. Supposing his venture 1*l.*, the prize for success should be 90*l.* By reducing it to 70*l.* the lottery-keeper reduces the real value of the ticket from 1*l.* to one-nineteenth part of 70*l.*, or to 15*s.* $6\frac{2}{3}$*d.*, defrauding the speculator of two-ninths of his stake. Such a venture as this is called a *determinate drawing*.

The next venture allowed in the Geneva system is called *simple ambe*. Two numbers are chosen. If both these appear among the five drawn, the prize is 270 times the stake. Now among the 90 numbers the player can select two, in 8,010 different ways; for he can first take any one of the 90 numbers, and then he can take for his second number any one of the 89 numbers left; that is, he may make 90 different first selections, each leaving him a choice of 89 different second selections; so that there are 90 times 89 (or 8,010) possible selections in all. But in any set of five numbers there are, treating them in the same way, only 20 (or 5 times 4) different arrangements of two numbers. So that out of 8,010 possible selections only 20 appear in each

drawing of five numbers. The speculator's chance then is only 20 in 8,010 or 2 in 801; and he ought, if he wins, to have for prize his stake increased in the ratio of 801 to 2, or 400½ times. Instead of this it is increased only 270 times. At each venture he receives in return for his stake a chance worth less than his stake, in the same degree that 270 is less than 400½; he is, in fact, defrauded of nearly one-third the value of his stake.

The next venture is called *determinate ambe*. Here the speculator names the order in which two selected numbers will appear. Instead of 20 chances at any drawing of five numbers, he has only one chance—one chance in 8,010. He ought then to receive 8,010 times his stake, if he wins. As a matter of fact he receives only 5,100 times his stake. From this it follows that he is defrauded of 2,910 out of 8,010 parts of his stake, or very nearly three-eighths of the stake's value.

But more speculative ventures remain. The speculator can name three numbers. Now there are 704,880 possible selections of three numbers out of 90. (There are 8,010 possible selections of two numbers, as already shown, and with each of these any one of the remaining 88 numbers can be taken to make the third number; thus we have 88 times 8,010, or 704,880 sets of three numbers in all.) These can appear among the five drawn numbers in 60 different ways (5 times 4 times 3). Thus the speculator has 60 chances out of 704,880, or one chance in 11,748. He ought then to receive 11,748 times his stake, if he wins; but in reality he

receives only 5,500 times his stake in this event. Thus the lottery-keeper robs him of more than half of his just winnings, if successful, and of more than half the mathematical value of his stake at the outset. The venture in this case is called *simple terne*. *Determinate terne* is not allowed. If it were, the prize of a successful guess should be 704,880 times the stake.

Quaterne involves the selection of four numbers. With 90 numbers, 61,334,560 (704,880 times 87) different selections of four numbers can be made. Among the five drawn numbers there can only be found 120 arrangements of four numbers. Thus the speculator has only 120 chances out of 61,334,560, or one chance out of 511,038. He ought therefore, if he wins, to receive 511,038 times his stake. The prize is only 75,000 times the stake. The lottery-keeper deducts, in fact, six-sevenths of the value of the stake at each venture. *Determinate quaterne* is, of course, not admitted.

Simple *quaterne* is, at present, the most speculative venture adopted. Formerly *quine* was allowed, the speculator having five numbers, and, if all five were drawn, receiving a million times the value of his stake. He should have received 43,949,268 times its value; so that, in effect, he was deprived of more than 42 forty-thirds of the true value of his venture.

The following table shows the amount by which the terms of the Geneva system reduce the value of the stake in these different cases, the stake being set at 1*l.* for convenience:

	Actual Worth of 1*l.* Stake	Robbery per 1*l.* Stake
	s. *d.*	*s.* *d.*
Simple drawing . . .	16 8	3 4
Determinate drawing . .	15 $6\frac{3}{4}$	4 $5\frac{1}{4}$
Simple ambe . . .	13 6	6 6
Determinate ambe . .	12 9	7 3
Terne	9 $4\frac{1}{2}$	10 $7\frac{1}{2}$
Quaterne	2 $11\frac{1}{4}$	17 $0\frac{3}{4}$

It may be thought, perhaps, that such speculative ventures as terne and quaterne would very seldom be made. But the reverse was the case. These were the favourite ventures; and that they were made very often is proved to everyone acquainted with the laws of chance by the circumstance that they not unfrequently proved successful. For every time such a venture as a simple quaterne was won, it must have been lost some half a million times.

It appears that in France the Geneva system was adopted without any of the limitations we have mentioned, and with some additional chances for those who like fanciful ventures. Professor De Morgan, in his 'Budget of Paradoxes' says:—'In the French lottery five numbers out of ninety were drawn at a time: any person, in any part of the country, might stake any sum upon any event he pleased, as that 27 should be drawn; that 42 and 81 should be drawn; that 42 and 81 should be drawn, and 42 first; and so on up to a *quine determiné*, if he chose, which is betting on five given numbers in a given order.' The chance of a successful guess, in this last case, is 1 in 5,274,772,160. Yet if every grown person in Europe made one guess a day

venturing a penny on the guess, and receiving the just prize, or say 4,800,000,000 times his stake, on winning, it would be practically certain that in less than a year some one would win 20,000,000*l.* for a penny! It would be equally certain that though this were repeated dozens of times, the lottery-keepers would gain by the arrangement, even at the rate above stated. Nay, the oftener they had to pay 20,000,000*l.* for a penny the greater their gains would be. As the actual prize in such a case would be 10 million instead of merely 5,275 million times the stake, their real gains, if they had to pay such prizes often, would be enormous. For, in the long run, every prize of half a million pounds for a shilling stake would represent a clear profit of 250 million pounds. The successful ventures would be only 1 in about 5,000 millions of unsuccessful ones, while paid for only at the rate of 10 million stakes.

No instances are on record of a *quine determiné* being won, but a simple *quine*, the odds against which, be it remembered, are nearly 44 millions to 1, has been won; and simple *quaternes*, against which the odds are more than half a million to 1, have often been won. In July 1821 a strange circumstance occurred. A gambler had selected the five numbers 8, 13, 16, 46, and 64, and for the same drawing another had selected the four numbers 8, 16, 46, and 64. The numbers actually drawn were

8 46 16 64 13

so that both gamblers won. Their stakes were small,

unfortunately for them and fortunately for the bank, and their actual winnings were only 131,350 francs and 20,852 francs respectively. If each had ventured 1*l.* only, their respective winnings would have been 1,000,000*l.*, and 75,000*l.* The coincidence was so remarkable (the antecedent probability against two gamblers winning on a simple drawing or simple *quine* and a simple *quaterne* being about 22 billions to 1), that one can understand a suspicion arising that a hint had been given from some one employed at the lottery-office. M. Menut insinuates this, and a recent occurrence at Naples suggests at least the possibility of collusion between gamblers and the drawers of lottery numbers. But in the case above cited the smallness of the stakes warrants the belief that the result was purely accidental. Certainly the gamblers would have staked more had they known what was to be the actual result of the drawing. The larger winner seems to have staked two sous only, the prize being, I suppose, 1,313,500 times the stake, instead of 1,000,000 as on a similar venture in the Geneva lottery. Possibly the stake was a foreign coin, and hence the actual value of the prize was not a round number of francs. The smaller winner probably staked five sous or thereabouts in foreign coin.

Simple *quaternes*, as we have said, occurred frequently in France. De Morgan remarks that the enormous number of those who gambled 'is proved to all who have studied chances arithmetically by the numbers of simple *quaternes* which were gained: in 1822, fourteen; in 1823, six; in 1824, sixteen; in

1825, nine, &c.' He does not, however, state the arithmetical proportion involved. If we take the average number at ten per annum, it would follow that about five million persons per annum staked money on this special venture—the simple *quaterne*—alone. Quetelet states that in the five years 1816-1820, the total sums hazarded on all forms of venture in the Paris lottery amounted to 126,944,000 francs—say 5,000,000*l.* The total winnings of the speculators amounted to 94,750,000 francs—say about 3,790,000*l.* The total amount returned to the treasury was 32,194,000 francs, or about 1,288,000*l.*, a clear average profit of 257,600*l.* per annum. Thus the treasury received rather more than a fourth of the sum hazarded. The return to the speculators corresponded nearly to that which would have been received if all the ventures made had been on a determinate single number.

In all these methods, the greater the number of speculators the greater the gains of those who keep the lottery. The most fortunate thing which can happen to the lottery-keepers is that some remarkably lucky hit should be made by a speculator, or a series of such hits. For then they can advertise the great gains made by a few lucky speculators, saying nothing of the multitudes who have lost, with the result that millions are tempted to become speculators. There is this great advantage in the Geneva system: that the total number of losers can never be known except to the lottery-keepers. In the old-fashioned English system the number of losers was as well known as the number of winners and their

respective gains. But the keepers of the Paris and Geneva lotteries, as of those which have since been established on the same system, could publish the lists of winners without any fear that newspaper writers or essayists would remind the general public of the actual number of losers. The student of probabilities might readily calculate the probable number of losers, and would be absolutely certain that the real number could not differ greatly from that calculated; but he could not definitely assert that so many had lost, or that the total losses amounted to so much.

It occurred to the Russian Government, which has at all times been notably ready to take advantage of scientific discoveries, that a method might be devised for despoiling the public more effectually than by the Geneva method. A plan had been invented by those who wanted the public money, and mathematicians were simply asked to indicate the just price for tickets, so that the Government, by asking twice that price, or more, might make money safely and quickly. The plan turned out to be wholly impracticable; but the idea and the result of its investigation are so full of interest and instruction that I shall venture to give a full account of them here, noting that the reader who can catch the true bearing of the problem involved may consider himself quite safe from any chance of being taken in by the commoner fallacies belonging to the subject of probabilities.

The idea was this:—Instead of the drawing of numbers, the tossing of a coin was to decide the prize

to be paid, and there were to be no blanks. If 'head' was tossed at a first trial the speculator was to receive a definite sum—2*l.* we take for convenience, and also because this seems to have been nearly the sum originally suggested in Russian money. If 'head' did not appear till the second trial the speculator was to receive 4*l.*; if 'head' did not appear till the third trial, he received 8*l.*; if not till the fourth, he received 16*l.*; if not till the fifth, 32*l.*; till the sixth, 64*l.*; the seventh 128*l.*; the eighth, 256*l.*, and so on; the prize being doubled for each additional tossing before 'head' appeared. It will be observed that the number of pounds in the prize is 2 raised to the power corresponding to the number of that tossing at which 'head' first appears. If it appears first, for instance, at the tenth trial, then we raise 2 to the 10th power, getting 1,024, and the prize is 1,024*l.*; if 'head' appears first at the twelfth trial, we raise 2 to the 12th power, getting 4,048, and the prize is 4,048*l.*

Doubtless the origin of this idea was the observed circumstance that the more speculative ventures had a great charm for the common mind. Despite the enormous deduction made from the just value of the prize, when *ternes*, *quaternes*, and other such ventures were made, the public in France, Switzerland, and Italy bought these ventures by millions, as was shown by the fact that several times in each year even *quaternes* were won. Now in the Petersburg plan there was a chance, however small, of enormous winnings. Head might not appear till the tenth, twelfth, or even the twentieth

tossing; and then the prize would be 1,024*l.*, 4,048*l.*, or 1,048,576*l.*, respectively. It was felt that tens of millions would be tempted by the chance of such enormous gains; and it was thought that the gains of Government would be proportionately heavy. All that was necessary was that the just value of a chance in this lottery should be ascertained by mathematicians, and the price properly raised.

Mathematicians very readily solved the problem, though one or two of the most distinguished (D'Alembert, for instance) rejected the solution as incomprehensible and paradoxical. Let the reader who takes interest enough in such matters pause for a moment here to inquire what would be a natural and probable value for a chance in the suggested lottery. Few, we believe, would give 10*l.* for a chance. No one, we are sure—not even one who thoroughly recognised the validity of the mathematical solution of the problem—would offer 100*l.* Yet the just value of a chance is greater than 10*l.*, greater than 100*l.*, greater than any sum which can be named. A Government, indeed, which would offer to sell these chances at say 50*l.* would most probably gain, even if many accepted the risk and bought chances—which would be very unlikely, however. The fewer bought chances the greater would be the Government's chance of gain, or rather their chance of escaping loss. But this, of course, is precisely the contrary to what is required in a lottery system. What is wanted is that many should be encouraged to buy chances, and that the more chances are bought the

greater should be the security of those keeping the lottery. In the Petersburg plan, a high and practically prohibitory price must first be set on each chance, and even then the lottery-keepers could only escape loss by restricting the number of purchases. The scheme was therefore abandoned.

The result of the mathematical inquiry seems on the face of it absurd. It seems altogether monstrous, as De Morgan admits, to say that an infinite amount of money should in reality be given for each chance, to cover its true mathematical value. And to all intents and purposes any very great value would far exceed the probable average value of any possible number of ventures. If a million million ventures were made, first and last, 50*l.* per venture would probably bring in several millions of millions of pounds clear profit to the lottery-keepers; while 30*l.* per venture would as probably involve them in correspondingly heavy losses: 40*l.* per venture would probably bring them safe, though without any great percentage of profit. If a thousand million ventures were made, 30*l.* per venture would probably make the lottery safe, while 35*l.* would bring great gain in all probability, and 25*l.* would as probably involve serious loss. If all the human beings who have ever lived on this earth, during every day in their lives had been taking chances in such a lottery, the average price of all the sums gained would be quite unlikely to approach 100*l.* Yet still the mathematical proposition is sound, that if the number of speculators in the Petersburg lottery were absolutely unlimited, no sum,

however great, would fairly represent the price of a chance. And while that unpractical result (for the number of speculators would not be unlimited) is true, the practical result is easily proved, that the larger the number of venturers the greater should be the price for each chance—a relation which absolutely forbids the employment of this method of keeping lotteries.

Let us see how this can be shown. De Morgan has given a demonstration, but it is not one to be very readily understood by those not versed in mathematical methods of reasoning. I believe, however, that the following proof will be found easy to understand, while at the same time satisfactory and convincing.

Suppose that eight ventures only are made, and that among the eight, four, or exactly half, toss head the first time; of the remaining four, two half-toss head at the second trial; of the remaining two, one tosses head at the third trial; while the other tosses head at the fourth trial. This may be regarded as representing what might on the average be expected from eight trials, though in reality it does not; for of course, if it did, the average price per chance, inferred from eight such trials, would be the true average for eight million trials, or for eight million times eight million. Still it fairly represents all that could be hoped for from a single set of eight ventures. Now we see that the sums paid in prizes, in this case, would be four times 2*l.* for those who tossed 'head' at the first trial; twice 4*l.* for those who tossed 'head' at the second trial; 8*l.* for him who tossed 'head' at the

third trial; and 16*l.* for the last and most fortunate of the eight; or 40*l.* in all. This gives an average of 5*l.* for each chance.

Now suppose there are sixteen ventures, and treat this number in the same way. We get eight who receive 2*l.* each; four who receive 4*l.* each; two who receive 8*l.* each; one who receives 16*l.*; and one who receives 32*l.* The total, then, is 96*l.*, giving an average of 6*l.* for each chance.

Next take thirty-two ventures. Sixteen receive 2*l.* each; eight 4*l.* each; four 8*l.* each; two 16*l.* each; one 32*l.*; and one 64*l.*; a total of 224*l.*, giving an average of 7*l.* for each venture.

It will be noticed that the average price per venture has risen 1*l.* at each doubling of the total number of speculators. Nor is it difficult to perceive that this increase will proceed systematically. To show this we take a larger number, 1,024, which is 2 doubled ten times, or technically 2 raised to the 10th power. Treating this like our other numbers, we find that 512 speculators are to receive 2*l.* each, making 1,024*l.* in all; thus we get as many pounds as there are ventures for this first halving. Next 256 receive 4*l.* each, or 1,024 in all; that is, again we get as many pounds as there are ventures, for this second halving. Next, 128 receive 8*l.*, or 1,024*l.* in all; or again, we get as many pounds as there are ventures, for this third halving. This goes on ten times, the tenth halving giving us one speculator who receives 1,024*l.*, and still leaving one who has not yet tossed 'head.' Since each halving

gives us 1,024*l.*, we now have ten times 1,024*l.* The last speculator tosses 'head' at the next trial and wins 2,048*l.*; making a grand total of twelve times 1,024*l.*, or twelve times as many pounds as there are speculators. The average, therefore, amounts to 12*l.* per chance; and we see, by the way in which the result has been obtained, that in every such case the chance will be worth 2*l.* more than as many pounds as there are halvings. Of course the number of halvings is the number representing the power to which two is raised to give the number of speculators. The number of speculators need not necessarily be a power of 2. We have only supposed it so for simplicity of calculation. But the application of the method of halving can be almost as readily made with any number of speculators. It is only when we get down to small numbers, as 9, 7, 5, or 3, that any difficulty arises from fractional or half men; but the result is not materially affected where the original number is large, by taking 4 or 3 as the next halving after either 7 or 9 (for example), or 2 as the next halving after 3. But practically we need not carry out these halvings, after we have once satisfied ourselves of the validity of the general rule. Thus, suppose we require to ascertain a fair value for a million chances. We find that the nearest power of 2 to the number one million is the 20th: 22*l.*, then, is a fair value.

But of course, the whole train of our reasoning proves that while probably 22*l.* would be a fair value for a million ventures, it could not be the mathematically just value. For who is to assure the lottery-keeper

that after the million ventures, another million will not be taken? Now for two million ventures the probable value according to our method would be 23*l.*, since two millions is nearly equal to 2 raised to the 21st power. There might be a million million ventures; and if 22*l.* were really the true price for one million, it would be the true price for each of the million ventures. But since a million million are roughly equal to 2 raised to the 40th power, the price according to our method would be about 42*l.* per chance.

All that can be said is that among any definite number of trials it is not antecedently probable that there will be any of those very long runs of 'trials' which are practically certain to occur when many times that number of trials (whatever it may be) are made.

The experiment has been actually tried, though it was not necessary to establish the principle. So far as the relatively small average value of the chance, when a few ventures only are made, the reader can readily try the experiment for himself. Let him make, for instance, eight trials, each trial ending when he has tossed head; and according as head comes at the first, second, or third, &c. tossing in any trial, let him write down 2*l.*, 4*l.*, 8*l.*, &c., respectively. The total divided by eight will give the average value of each trial. Buffon and each of three correspondents of De Morgan's made 2,048 trials—an experiment which even the most enthusiastic student of chances will not greatly care to repeat. Buffon's results, the only set we shall separately quote, were as follows. In 1,061 trials, 'head'

showed at the first tossing; in 494, at the second; in 232, at the third; in 137, at the fourth; in 56, at the fifth; in 29, at the sixth; in 25, at the seventh; in 8, at the eighth; in 6, at the ninth. The 2,048 trials, estimated according to the Petersburg system, would have given 20,114*l.* in all, or nearly 10*l.* per game. According to our method, since 2,048 is the eleventh power of 2*l.*, the average value of each chance would be 13*l.*;[1] and Buffon's result is quite as near as could be expected in a single experiment on 2,048 trials.

But when we take the four experiments collectively, getting in this way the results of 8,192 trials (of which De Morgan, strangely enough, does not seem to have thought), we find the average value of each chance

[1] I note that De Morgan obtains the value 11*l.* instead of 13*l.* But he strangely omits one of the last pair of trials altogether. Thus, he says, 'in the long run, and on 2,048 trials, we might expect two sets in which "heads" should not appear till the tenth throw,' which is right, 'and one in which no such thing should take place till the eleventh,' which is also right. But it is because there will probably be four trials of which two only will probably give 'heads,' that we may expect two to give 'tails' yet once more. The two which gave 'heads' are the two first mentioned by De Morgan, in which 'heads' appear at the tenth throw. Of the two remaining we expect one to give 'head,' the other 'tail.' The former is the 'one' next mentioned by De Morgan, in which 'head' appears at the eleventh throw. The other in which 'tail' may be expected to appear is the most valuable of all. Even if 'head' appears at the next or twelfth tossing, this trial brings a prize worth twice as many pounds as the total number of trials—and therefore adds 2*l.* to the average value of each trial. It is quite true that Buffon's experiment chances to give a result even less than De Morgan's value, and still further therefore from mine. But as will be seen, the other experiment gave an average result above his estimate, and even above mine. It cannot possibly be correct to omit all consideration of the most profitable trial of all.

greatly increased, as theory requires—and, as it happens, increased even beyond the value which theory assigns as probable for this number of trials. Among them there was only one in which head appeared after tail had been tossed 11 times, whereas we might expect that there would be four such cases; but there was one case in which head only appeared after tail had been tossed 13 times, and there were two cases in which head only appeared after tail had been tossed 15 times. Of course this was purely accidental. We may always be tolerably sure that in a large number of tossings, about one-half will be head and about one-half tail. But when only a few tossings are to be made, this proportion can no longer be looked for with the same high degree of probability. When, again, only four or five chances are left, we may find these all dropping off at once, on the one hand, or one or two of them may run on with five or six more successful tossings; and as at each tossing the prize, already amounting for the last trial to as many pounds as there were originally chances, is doubled, we may find the average price of each chance increased by 1*l.*, 2*l.*, 4*l.*, 8*l.*, 16*l.*, or more, by the continued success of the longest lasting trial, or perhaps of two or three lasting equally long. This happened in the 8,192 trials whose results are recorded by De Morgan. I find that the total amount which would have been due in prizes, according to the Petersburg plan, would have been 150,830*l.*, an average of 18*l.* 8*s.* 2½*d.* (almost exactly) per trial; whereas the average for 8,192 trials on my plan would be only 15*l.*

It is manifest that, though in a million trials by this method some such sum as 30*l.* per trial would probably cover all the prizes gained, it would be unsafe to put any definite price on each venture, where the number of venturers would of necessity be unlimited. And since even a price which would barely cover the probable expenses would be far more than speculators would care to give, the plan is utterly unsuited for a public lottery. It may be well to note how large a proportion of the speculators would lose by their venture, even in a case where the total ventured was just covered by the prizes. Suppose there were rather more than a million speculators (more exactly, that the numbers were the 20th power of 2, or 1,048,576), and that the average result followed, the price per venture being 22*l.* Then 524,288 persons would receive only 2*l.* and lose 20*l.* each; 262,144 would receive only 4*l.* and lose 18*l.* each; 131,072 would receive 8*l.* and lose 14*l.* each; 65,536 would receive 16*l.* and lose 6*l.* each. All the rest would gain; 32,768 would receive 32*l.* and gain 10*l.* each; 16,384 would receive 64*l.* and gain 42*l.* each; and so on; 8,192 would receive 128*l.* each; 4,096 would receive 256*l.* each; 2,048 each 512*l.*; 1,024 each 1,024*l.*; 512 each 2,048*l.*; 256 each 4,096*l.*; 128 each 8,192*l.*; 64 each 16,384*l.*; 32 each 32,768*l.*; 16 each 65,536*l.*; 8 each 131,072*l.*; 4 each 262,144*l.*; 2 each 524,288*l.*; 1 would receive 1,048,572*l.*; and lastly, one would receive 2,097,952*l.* But there would be only 65,536 out of 1,048,576 speculators who would gain, or only 1 in 16.

It is singular that whereas it would be almost impossible to persuade even one person to venture 22*l.* in such a lottery as we have described, almost any number of persons could be persuaded to join again and again in a lottery where the prizes and blanks were arranged as in the way described in the preceding paragraph as the average outcome of 1,048,576 ventures. In other words, no one puts so much faith in his luck as to venture a sum on the chance of gaining a little if he tosses 'tail' four times running (losing if 'head' appears sooner), and of gaining more and more the oftener 'tail' is tossed, until, should he toss tail 20 times running, he will receive more than two million pounds. But almost every person who is willing to gamble at all will be ready to venture the same sum on the practically equivalent chance of winning in a lottery where there are rather more than a million tickets, and the same prizes as in the other case. Whatever advantage there is, speaking mathematically, is in favour of the tossing risk; for the purchaser of a trial has not only the chance of winning such prizes as in a common lottery arranged to give prizes corresponding to the above-described average case, but he has a chance, though a small one, of winning four, eight, sixteen, or more millions of pounds for his venture of 22*l.* We see then that the gamblers are very poor judges of chances, rejecting *absolutely* risks of one kind, while accepting *systematically* those of another kind, though of equal mathematical value, or even greater.

In passing, I may note that the possibility of win-

ning abnormally valuable prizes in the Petersburg lottery affords another explanation of the apparent paradox involved in the assertion that no sum, however large, fairly represents the mathematical value of each trial. To obtain the just price of a lottery-ticket, we must multiply each prize by the chance of getting it, and add the results together; this is the mathematical value of one chance or ticket. Now in the Petersburg lottery the possible prizes are 2*l.*, 4*l.*, 8*l.*, 16*l.*, and so on, doubling to infinity; the chances of getting each are, respectively, one-half, one-fourth, one-eighth, one-sixteenth, and so on. The value of a chance, then, is the half of 2*l.*, added to the quarter of 4*l.*, to the eighth of 8*l.*, and so on to infinity, each term of the infinite series being 1*l.* Hence the mathematical value of a single chance is infinite. The result appears paradoxical; but it really means only that the oftener the trial is made, the greater will be the probable average value of the prizes obtained. Or, as in fact the solution is that if the number of trials were infinite the value of each would be infinite, we only obtain a paradoxical result in an impossible case. Note also that the two kinds of infinity involved in the number of trials and in the just mathematical price of each are different. If the number of trials were 2 raised to an infinitely high power, the probable average value of each trial would be the infinitely high number representing that power. But 2 raised to that power would give an infinitely higher number. To take very large numbers instead of infinite numbers, which simply elude us:—Suppose the number

of trials could be 2 raised to the millionth power; then the probable average value of each would be 1,000,002*l.*, which is a large number of pounds; but the number is a mere nothing compared with the number of trials, a number containing 301,031 *digits*! If the smallest atom, according to the estimate made by physicists, were divided into a million millions of parts, the entire volume of a sphere exceeding a million million times in radius the distance of the remotest star brought into view by Lord Rosse's mighty telescope would not contain a million millionth of that number of these indefinitely minute subdivisions of the atom. Nay, we might write trillions or quadrillions where we have just written millions in the preceding lines, and yet not have a number reaching a quadrillionth part of the way to the inconceivable number obtained by raising 2 to the millionth power. Yet for this tremendous number of trials the average mathematical value of each would amount but to a poor million—absolutely nothing by comparison.

GAMBLING IN SHARES.

If there is any evil quality of human nature which, by its persistence, its wide-spreading and its mischievous influence, speaks of the inborn savagery of human nature, it is the greed for chance-won wealth. In all ages men have been moved by it. It has seemed so natural, that men have lost sight of its innate immorality. 'If I take my chance fairly with others and win,' the gambler argues, 'I have done no man wrong, not even myself or the members of my family. What I win I can regard as gain, not less legitimate than the profits on some business transaction. If other men are ruined, or if I run the risk of ruin myself, this is no more than happens all the time. Other men may be killed in various chance ways; I may myself be killed ere the day is out in some chance manner: why should I not, since I and others must incur the chances of life, raise other chance issues by which either gain or loss may result to others or to myself?'

It may be that false though this reasoning is as a defence, there is more of excuse in it than those imagine who use it. Beyond doubt the element of chance which enters into all lives, has had a most potent influence in

moulding the characters of all men. If we consider the multitudinous fancies and superstitions of men like sailors, farmers, and hunters, whose lives depend more on chance than those of men in other employments, and recognise this as the natural effect of the influence which chance has on their fortunes, we need not wonder if the influence of chance in moulding the minds and characters of our ancestors during countless generations, should have produced a very marked effect on human nature. An immense number of those from whom I (for instance) inherit descent, must in the old savage days have depended almost wholly on chance for the very means of subsistence. When 'wild in wood' the savage (very far, usually, from being noble) ran, he ran on speculation. He might or might not be lucky enough to earn his living on any day by a successful chase, or by finding such fruits of the earth as would supply him with a satisfactory amount of food. He might have as much depending on chances which he could not avoid risking, as the gambler of to-day has when he 'sees red' and stakes his whole fortune on a throw of the dice or a turn of the cards. We cannot be doubtful about the effects of such chance influences on even the individual character. Repeated generation after generation they must have tended to fill men with a gambling spirit, only to be corrected by many generations of steady labour; and unfortunately, even in the steadiest work the element of chance enters largely enough to render the corrective influence of such work on the character of the race (as distinguished from the

individual) much slower than it might otherwise be. Every man who has to work for a living at all, every man who has to depend in any way on business for wealth (which is different from working for a living) has to trust more or less to chance in many respects. So that nearly all men have their characters in some degree modified by this peculiarity of their environment. The inherited tendency of each one of us towards gambling, in some one or other of its multitudinous forms, is undoubtedly strengthened in this way; though fortunately it may be much more than correspondingly weakened by training, by thought, and by steady pursuance of life's proper work.

That gambling is immoral has been recognised by those who have noticed the effects of established lottery systems, or of gambling establishments such as formerly were allowed to flourish in our cities, to the demoralisation and ruin of thousands—among rich and poor alike. Governments which once originated lotteries, and reaped large profit from them, now not only cease to raise money in so iniquitous a manner, but forbid lotteries, and, as far as they can, prevent them. That they remain an attraction for an immense number of our people is shown by the circumstance that lotteries permitted on the Continent advertise largely in English newspapers and periodicals, and that their circulars reach thousands of Englishmen through the post. I have myself had experience of the assiduities of Continental lottery promoters in both forms, having received dozens of invitations to invest in these demoralising

ventures, and having also had any number of advertisements offered for 'Knowledge.' Yet every lottery system, when it comes to be examined, proves, as I have shown in essays on lotteries, to be based on fraud—in such sort as to bring sure gains to the promoters of the lottery, sure loss in the long run to the purchasers of tickets—sure ruin even, if they will but avail themselves in sufficient degree of the opportunities for ruin obligingly proffered them.

In England, fortunately, lotteries are illegal. Yet a method has been devised by which all classes of the community may court fortune or ruin in the freest manner, without gambling on card games (which would attract attention and be unsuitable for those who object to notoriety) or entering on turf speculations (still more unpleasantly conspicuous in their method). I know not that at the worst gambling-hells in the bad old times of the Georges fortunes (and, what is worse, not fortunes alone, but competencies and pittances) could be more readily squandered than by the various forms of speculation in stocks now made of easy access and convenient procedure for all classes of our people—for men, for women, and even for those who are little more than children.

Speculation on the Stock Exchange has, of course, been always a recognised method of gambling. In such speculation as in the system now invitingly offered to all classes there was often, if not generally, very little money behind the speculations, compared with the amount actually supposed to be invested in the various

transactions. (I use the word 'supposed' in an entirely conventional sense, for in Stock Exchange speculations nothing is supposed to be actually invested, though such and such amounts of stock are named as bought or sold.) A speculator need be prepared only to pay the difference between the value of the stock he is supposed to have bought or sold at the beginning of the time-bargain and its diminished or increased value when the time expires. Thus a man shall nominally buy 10,000*l.* in certain stocks at, we will say, 9,927*l.*, which at the end of the time for which the shares are supposed to have been bought, shall be worth only 9,811*l.*; in that case, apart from brokerage or commission, he loses 116*l.* on the transaction. Or, if he had sold stock at 9,927*l.*, nominally (not really possessing any such amount), and its value rose to 10,033*l.* at the time for which the bargain was entered on, then he would lose 106*l.* It is only (as a rule) some such proportion as this of the large sum bought or sold that he will actually lose if unfortunate, or gain if he has luck, on a transaction which has such imposing dimensions.

The system, however, by which gambling in stocks is now made accessible to all is more inviting than the system of time-bargains.

By the time-bargain system a man could not tell how much he was risking, any more than he could tell how much he might gain. When settling time came he might have won much or little, or he might have lost little or much, on any particular speculation. The probable gains and the probable losses, apart

from special knowledge or supposed knowledge of the chances of rise or fall in price, were evenly balanced.

Now, though this might do very well for men on 'Change, just as hard gamblers in the good old times were well content to risk their money on the pulling of a straw or the toss of a die, risks of this sort have no attraction for the average gamblers of the ordinary type. If the history of men who have lost largely on the turf were known, it would be found that, for one case where the loss has arisen from wagers on even terms, there will be a thousand or probably an even larger number in which men have been ruined by backing horses at odds. What the average gambler, who is nearly always a weakling, wants, is a chance of winning a large sum by risking a small one. If he backs a horse at odds he is well pleased. But then the horse must also be a favourite, or at least he must himself have a high opinion of the horse's chance. Now a horse cannot be a favourite and also have the odds against him, unless there is a good field. Hence, the average betting man of the pigeon type likes to lay his money on one or other of the favourites in a large race, where the odds are at least four or five to one against even the chief favourite. Then if he loses he loses but a small sum compared with that which he has a chance (and, as he thinks, almost a certainty) of gaining. The bookmaker, as we have seen, takes advantage of this delusion. He is aware that a man who, knowing little about horses, fancies a particular horse—on the strength, perhaps, of false information which the bookmaker himself may have

helped to spread—will not be careful to note whether the precise odds are offered. If the current odds are 12 to 1, the simpleton will be content with 11 or 10 to 1, or even less. The bookmaker, then, acts on the contrary principle. He always, or nearly always, lays the odds against horses—he seems to risk much to gain little—but, on the plan he actually follows of always offering less than the fair odds, his multiplied little gains nearly always outbalance heavily his occasional heavy losses. We occasionally hear of a large bookmaker coming to grief; but not often, not nearly so often, as one could wish.

Seeing that such are the ways of the gambling public, it will be seen that the method of gambling followed by men on 'Change would not be seductive enough for the general public. Those who live on the weakness of men for gambling very soon found this out. Although some among them tried to make the Stock Exchange system of speculating generally available, the public, as a whole, were never greatly attracted by a method of making a fortune which seemed to them both slow and dangerous.

But a system is in vogue now which is as seductive as any lottery system, is at present safe (strangely enough) from check or punishment, and insures a splendid profit from the foolish folk who take part in it, even from those who win money by it—as, for a time, the speculators often do.

This system, which men on 'Change by no means like for their own transactions, is that called 'the cover

system'; as a method of courting ruin it is the perfection of simplicity.

In the cover system each transaction is closed, not when a certain time but when a certain money limit is reached (though in each, at a price, the transaction may be extended). The speculator—the victim we may call him, gaily though he trips up the altar steps—pays a certain sum to a stockbroker of a certain class, as a 'cover' or deposit upon a hundred times that amount in some stock which he fancies, or thinks he knows, will rise or fall in price. He may be either a buyer or a seller (always nominally), either a bull or a bear. It is not necessary, if he is a buyer, that there should be any real seller, or, if he is a seller, that there should be any real buyer. Nothing is necessarily bought or sold—(except the speculator himself, who is both). The account having been now opened for that particular stock, all that has to be done is to wait until the account can be closed at a profit.

If stock has been nominally bought, the speculator waits for it to rise, so that when it has risen high enough he may close the account and gather in his gains; or, if stock has been sold, he waits in like manner until it shall fall. When it is rising or falling to his advantage he is in pleasing doubt whether the time has arrived to close to the greatest attainable advantage. If he waits too long and it begins to move the wrong way, he is apt to wait a little longer for the motion in that wrong direction to cease—often with disastrous results; but if he does not wait long enough,

and after the account has been closed the stock still continues to move in what would have been the right direction had the account been kept open, then he is made miserable by the thought that he has thrown away money which he might have gained. As he very seldom hits the precise moment when the greatest possible profit is to be reaped, he nearly always has the discomfort which arises from the thought that he has closed the account too soon or too late.

So much when fortune favours the speculator, as it very often does at the beginning. It is even said, and doubtless it is the case, that stockbrokers of the class we are considering, those who lend themselves to the gambling game which seems so inviting, take care that beginners who have plenty of money to lose, are led on by early successes. A poor fellow who cannot afford to lose more than a paltry ten or twenty pounds, and may even have had to borrow from his employer's till to get that, may be cleared out at once; but manifestly it would not do to dishearten a young fellow who has thousands to lose. Still, with one or the other, losing transactions have to be considered, sooner or later. Here the refined torture arising from anxiety as to the exact moment when the gain is as great as it is likely to be is wanting. The speculator scarcely ever troubles himself even to inquire when his loss, if he closes, is as great as he can reasonably let it be. So long as the loss is within the limit of the 'cover' he holds on. He may even, rather than lose the chance of a change of luck, extend the cover. But whenever his cover, whether left

unchanged or extended as far as he is prepared to go, is reached by the amount of loss, the account is closed and his deposit is forfeited.

Let us consider an actual transaction in detail; and that we may not in any way wrong the persons who attempt to mislead the more foolish part of the public in this matter, let us take an account published by one of themselves:—'"For instance, then"—says one of the most notorious of these in an advertisement published under guise of a story—"having reason to expect a certain stock (Great Westerns) is likely to go up" (the grammar I "expect, is likely" to be the stockbroker's own)—"the present price of which we will suppose is $132\frac{1}{4}$; a client sends 10*l.* 12*s.* 6*d.* as cover and commission, with instructions to buy 1,000*l.* Great Western Railway Stock." "If it goes up?" queried Captain Dayrell, becoming much interested. "Paying attention to the daily quotations, the operator notices that the stock rises, say, to $133\frac{1}{4}$–$\frac{1}{2}$, and 10*l.* or 10 per cent. is realised. If the stock rises to $134\frac{1}{4}$–$\frac{1}{2}$, 20*l.* or 20 per cent. is realised, and so on in proportion." "Should the reverse happen?" "If, however, contrary to expectation the stock goes down from $132\frac{1}{4}$ to $131\frac{1}{4}$–$\frac{1}{2}$, the cover has run off, and the transaction is closed with the loss of the 10*l.* cover only. Beyond this, and the commission of $\frac{1}{1\,6}$, or 12*s.* 6*d.* per 1,000*l.* stock, there is no further liability; and the beauty of the thing is, you only lose what cover you put up." "Suppose I put on more cover before it is too late?" "Then you can keep the account open," replied Roselle. "It is simple enough,

and very fair." "Yes; it limits the liability of the operator." "I see; he can choose any stock he pleases to operate in; and, if his judgment is sound, or the information good, the profit is certain." "Exactly," said Roselle, with a smile. "I can see. The profits may be very large, whilst the loss is always small," remarked the Captain.'

One can see tolerably well, I may remark in passing, how this account was written. The stockbroker for whom the series of stories was written (much as poetry used to be written for Moses & Son) sent to the writer, who uses the fine-sounding *nom de plume* of Bracebridge Hemyng, an example of the way of working the cover system, and this writer, whose stories fortunately are of the dull blood-and-thunder type, has simply turned the account into a dialogue, by breaking it up, and inserting 'the Captain said,' 'Roselle replied,' 'I see,' &c.

It will be noticed that in this account, and it is the same in all such accounts, nothing is said as to whence the money comes from by which, if the speculator wins, he gets his winnings. Many of these unfortunate gamblers have the idea that the stockbroker pays it out of his own pocket. It never seems to occur even to those who are not quite so foolish as to imagine this, that if the method of rapidly making large fortunes which stockbrokers advertise so freely, were as sure as they pretend, there would be very little rising stock for purchase by the outside public, and very little falling stock for sale to them. The stockbrokers would transact on their own behalf the business they are so eager to

transact for others for a consideration—the trifling brokerage of $\frac{1}{16}$ per cent.

If the real nature of the transaction were described, none but very foolish persons would enter on so transparently dangerous a course.

The stockbrokers of the particular class we are considering (for, of course, many stockbrokers are thoroughly respectable men) say to the moths, 'By risking so much you may gain large sums.' If they told the truth they would say, 'By paying in so much you enable me to purchase or sell such and such an amount of stock, at such and such sure profit through brokerage, without any risk.' The cover system has been devised to protect the stockbroker, not to profit the speculator.

Consider the position of the stockbroker in the case just described, after the sum of 10*l.* 12*s.* 6*d.* has been paid in. He purchases 1,000*l.* Great Western Railway Stock for his client, and watches the telegraphic tape. If the stock rises in value his client is able to close the account at a profit, and in that case will start a new account, with fresh brokerage, and be profitable to the stockbroker. Therefore it is better for the broker to have a lucky client, or even to give occasionally a useful hint in the beginning of a new client's career. But if the stock falls in value, the stockbroker, at the moment when the loss is equal in amount to the cover, closes the account, without loss to himself, and is the gainer by the brokerage.

But 'the brokerage is only $\frac{1}{16}$ per cent., and that is

a mere nothing.' If the brokerage were $\frac{1}{16}$ per cent. on the money risked by the speculator that might more reasonably be urged. It is, however, $\frac{1}{16}$ per cent. on a hundred times that amount. That is to say, it is not *one* but *one hundred* 16ths per cent., or $6\frac{1}{4}$ per cent. on the speculator's money.

If we compare the position of the speculator in such a transaction as this with that of a man who buys a ticket in a lottery, we shall be able to see in what position Stock Exchange speculation stands as compared with speculation in lotteries, admitted to be a losing business. In the case of stock gambling above considered, the speculator pays 12*s.* 6*d.* and risks 10*l.* for the chance of winning he knows not how much. With all consideration for his judgment or information, experience shows that we cannot really regard the stock as more likely to rise than to fall; and with any but 'wild cat' stock, with which no honest man can safely meddle,[1] it is altogether unlikely that the rise will be such as to give a profit of 50*l.* on the transaction. It is probably much more than a hundred to one against this. Now it is a hundred to one against the holder of one ticket out of a hundred in a lottery drawing the single prize of 500*l.* To get that chance he ought, strictly speaking, to pay only 5*l.*; but as the Louisiana lottery, and most others, are constituted he would probably have to pay about 10*l.* Here he has risked 10*l.* for the same

[1] As this remark might be misunderstood I explain that no man can safely speculate in bubble companies unless he is acquainted with the plans of the promoters—or, in other words, is as great a rascal as the promoters are.

chance of winning 500*l.* that the stock gambler in the other case has of winning 50*l.* True, the latter has the chance of winning some smaller sum; but, as a rule, the gambler in stocks never is content except with a large profit, of which he may boast as a fine stroke either of skill or luck.

If we compare the smallness of the amount risked with the sum which may be gained, all lotteries have a great and some have an immense advantage over Stock Exchange gambling. For five dollars or rather more than a pound, a gambler has in the Louisiana lottery the chance of winning 200,000 dollars.

Where the stock-gambling system seems to the dupes to have a great advantage over the lottery system is in the apparently small percentage of profit reaped by the person who manages the transaction. Just as players at *trente-et-un* used to imagine the advantage of the *refait* held by the bank so small as to leave the terms of the gambling all but even, and used to rejoice over the bank's small percentage of advantage on each transaction, so does the stockbroker's dupe, who would probably pay ten or twenty per cent., as the lottery gambler does, rather than not court ruin at all, rejoice at the nominal $\frac{1}{16}$ per cent. I have shown that in reality the percentage on the money risked is more than 6 per cent. It may be argued, and justly enough in a sense, that the risk of the cover-speculator who pays in 10*l.* is, in reality, precisely the same as the risk of a full speculator who actually bought 1,000*l.*'s worth of stock. The latter could stop short of a loss of 10*l.* just as

readily after buying the stock as the cover-speculator does at the outset. All he would have to do would be to watch the progress of prices, and sell so soon as the fall corresponded to a loss of 10*l.* At least he could do this in the case of far the greater number of stocks—in fact, this is practically what the stockbroker does for the cover-speculator. So that the percentage for brokerage is properly extended to the full amount. This is perfectly just in the case of a legitimate investment. But so soon as we consider how the cover-speculator renews and re-renews his risk on the smaller amount, we see that the percentage taken by the broker is very much more than $\frac{1}{16}$, much more even than $6\frac{1}{3}$ per cent. Like the Homburg bank's advantage on the *refait*, it is small on individual transactions, but mounts up to something enormous as a percentage, when considered with reference to the total amount of probable gain or loss after steady persistence in gambling.

Here, for example, is a case very favourable indeed for the speculator (who on the average would have no such luck):—A man pays 10*l.* as cover for 1,000*l.* stock, and 12*s.* 6*d.* the small percentage for the broker. He is lucky, and wins 20*l.* when the transaction is closed—say, in a week. This is splendid, for it is earning money, he thinks (stock gamblers always speak of earning money, just as racing men speak of bookmaking as if it were a respectable trade or profession), at the rate of 1,000*l.* a year. He now invests his 20*l.*, as cover on 2,000*l.*, in some other stock, either as buyer or seller, paying 1*l.* 5*s.* as brokerage. This time he is not

so fortunate; the stock moves the wrong way, and in the course, say, of another week his 20*l.* cover is forfeited. But, depending on a change of luck, and also feeling (as every gambler does) that he is essentially a lucky man, however at times fate may frown on him, he invests 10*l.* as cover, paying 12*s.* 6*d.* as brokerage on 1,000*l.*, winning in a week 20*l.* He invests—this wild kind of speculation may be pleasingly called investment—a sum of 15*l.* as cover, which in another week is forfeited, the brokerage being in this case 18*s.* 9*d.* He invests 16*l.* as cover, paying 1*l.* brokerage, and wins 16*l.*, in a week. He next invests 16*l.* again, paying 1*l.* again for brokerage, and forfeits his cover. Here we leave him, so far as further speculation is concerned, though it is very unlikely that he would stop his speculative system here. Let us consider what he has accomplished in the six weeks (we took always the same period of time, so that the summing-up might be simplified—about six weeks for the six transactions would have served equally well):

He has won 20*l.*, 20*l.*, and 16*l.*; he has lost 20*l.*, 15*l.*, and 16*l.* He has gained thus in all 5*l.* He has paid in brokerage 12*s.* 6*d.*, 1*l.* 5*s.*, 12*s.* 6*d.*, 18*s.* 9*d.*, 1*l.*, and 1*l.*—making a total of 5*l.* 8*s.* 9*d.* Therefore he is out of pocket 8*s.* 9*d.*; he has lost six weeks' use of the sum of 10*l.* first invested, to say nothing of a loss of the use of 5*l.* more during the third week; and he has undergone a good deal of worry and anxiety. Yet he has had better luck than he had a right to expect; for, on the whole, he has had a balance of gain over loss, so

far as the actual transactions have been concerned. If he had lost on the whole instead of gained he would still have lost in addition the sum of 5*l.* 8*s.* 9*d.* One may regard this as what he has paid for the privilege of investing 10*l.*, or thereabouts, during six weeks as cover on Stock Exchange speculations. He could not have carried on this preposterously foolish system of gambling without the kindly proffered assistance of the advertising class of stockbrokers; and that is what he has had to pay them for their ministrations. Regarded as percentage on 10*l.* for 6 weeks, it is at the rate of more than 470 per cent. per annum. This is the sort of percentage which 'the utterly insignificant brokerage' amounts to where the speculative wiseacre persists in his folly long enough. It is true the broker may not always reap such a profit in the year on any one victim—for the victim may be ruined before the year is out. But that is a misfortune amply repaired for the broker by the constant influx of fresh victims.

In the series of transactions imagined above the stockbroker, without risk, secures more than the victim would have gained if there had been no brokerage. He would have secured the same percentage had the investments been all ten times greater, or a hundred times greater, or a thousand, or ten thousand. Always he pockets freely and without risk; always, even the luckiest speculator pays freely, and the unluckiest speculator has to pay in like manner, besides losing heavily. Apart from success or failure in the speculations themselves—and in the long run these are bound to balance

themselves pretty equally unless the speculator gets 'tips,' in which case he is sure to lose heavily in the long run—the broker always makes a sure and large gain, the speculator always has a sure and large loss, in brokerage alone.

Of course the example I have just considered will not be regarded by the average speculative gambler as typical. He expects to win very much more than he loses, or to win always and not lose at all. In reality, he has no more right to expect a considerable balance of gain than a farmer has to expect exceptional weather. Assuming fair bargains, as I have pointed out in the preceding sections, the gambler in stocks has no right to expect to gain more than he loses. Of course he does expect to gain, or he would not speculate. But if he has a particle of common sense, he will see that at the best he can only gain on some transactions rather more than he loses on others. Hence such a result as I have considered above is about what might be expected to occur in the case of a lucky speculator.

Taking a more general view, a speculator would have reason to regard himself as exceptionally fortunate if his gains were to his losses in the proportion of nine to eight. Suppose now that a speculator went on for a whole year at this rate, gaining on the average 50*l.* a week; and suppose, further, that his gain, when he has gained, has averaged the amount of cover invested, his loss, when he has lost, being always the cover paid in. It will be seen that his full weekly gain has been 450*l.*,

his full weekly loss being 400*l.*; so that the total amount invested as cover has been 850*l.* weekly, the stock represented being 85,000*l.* The brokerage on this at $\frac{1}{16}$ per cent. amounts to 53*l.* 2*s.* 6*d.*; so that in this case, with a seeming gain of 50*l.* weekly, the unfortunate speculator loses 3*l.* 2*s.* 6*d.*, the broker pocketing all his client's gains and 3*l.* 2*s.* 6*d.* beyond.

Supposing, for a moment, all the transactions for one week, having the result just indicated for the winning speculator, to have been as between him and another, who has therefore been necessarily a losing speculator, we find that this other has had to pay *his* broker also 53*l.* 2*s.* 6*d.*, and has further lost 50*l.* He has lost, then, in all 103*l.* 2*s.* 6*d.*

Here, then, we have this result, calling the lucky speculator A, the unlucky speculator B, and two stockbrokers respectively R and S:

A has won 50*l.*, and paid 53*l.* 2*s.* 6*d.* in brokerage, being therefore only *minus* 3*l.* 2*s.* 6*d.*

B has lost 50*l.*, and paid 53*l.* 2*s.* 6*d.*, being therefore *minus* 103*l.* 2*s.* 6*d.*

A's broker R has gained 53*l.* 2*s.* 6*d.*

B's broker S has gained 53*l.* 2*s.* 6*d.*

So long as there are many idiotic A's and B's seeking their own ruin by the cover system, one need not necessarily assume that R and S stand appropriately for rascal and swindler. But when stockbrokers choose to join the ranks of those who advertise for clients of this sort, who confidently proclaim that speculation of this kind is a safe and ready way of making a fortune, and

thus ensnare thousands of foolish persons to enter on a path which leads always to loss and often to ruin and shame, they must be prepared to find themselves classed among creatures of prey. They are not the less wrong-doers that at present the law has not forbidden them to prey thus on the weak and foolish. The law should be altered and our gaols enlarged.

The defence is made that, if the speculator has good judgment or special information, he will win largely. The same defence has been made for the rascally system by which bookmakers devour the substance of the young and silly. Every man who gambles imagines he is trusting to his judgment, and that he has judgment in which to trust. From the foolish heir of 'noble' or wealthy family to poor stupid 'Arry, there is not a turf gambler of the pigeon type who does not think he can form a tolerably shrewd guess as to the chance of every favourite in a race, or that he has information which practically makes him safe to win. Repeated losses may, after a time, teach the sort of wisdom by which a man recognises his own inexperience; but even this is unusual.

Now, if the tyro cannot really form any idea as to the chances of a horse in a race, if the information to which he trusts is baseless or even misleading, can it be supposed that any, except the most experienced business men, can form a sound opinion about the points on which the ever-changing values of stock depend? Not one of those who speculate has in reality any sufficient power of *judging* in such matters at all; for sound business men never speculate. Nor would

the soundest judgment avail in the case of many kinds of stock, for the values change as the stock is 'pulled' by hands of whose very existence the ordinary speculator knows nothing.

If the ordinary speculator even had exceptional power of discrimination (an idea which is altogether absurd to those who know how foolish the ordinary speculator is), and if he always had special information on which he could rely (which again is absurd), his position would be altogether unsatisfactory. He would be less foolish but more knavish than I have been assuming. He would be much like a player in a card-game depending properly purely on chance, who should take advantage of exceptional keenness of sight or of information conveyed by a confederate to learn the cards held by the other players. For every pound one player or speculator gains through such judgment or information another player loses a pound, or several other players lose by amounts whose total is a pound.

It may be said that this is mere exaggeration, that it would apply to investment as closely as to speculation, or that it might even be applied to the ordinary transactions of trade, in which those who show good judgment and possess good experience succeed, while the unwise and inexperienced fail. In reality, it might as reasonably be said that wagering on a tradesman's chances of success or failure is as legitimate a way of trying to win money as carrying on trade, or that such wagering between a man who knows nothing about the

tradesman's chances and one who knows a great deal about them would be fair and honest.

This last comparison, by the way, is nearer the truth than probably most persons imagine. It is singular how little is understood about the real nature of stocks even by the speculative folk who imagine that they know all about them. Money invested in stock is in reality money lent, and usually money lent for business use. Of course Consols represent money lent to Government, while various foreign investments represent money lent to foreign Governments, and these can hardly be called business loans. But in the main the stocks dealt with in the business columns of our papers, the Foreign Market and City Intelligence, are loans to various companies engaged in commercial business.

Now, if we ask why these stocks vary as they do in value, from Consols down to the lowest class of stocks, we find that theoretically the changes correspond with the varying degrees of advantage or of security, or both combined, which the lender recognises in these different openings for lending his money.

A business company needing money for any particular purpose, and having good credit, will either borrow such and such a sum at a definite rate per cent. for interest, to be paid half-yearly or yearly, or else will nominally borrow a definite sum for a definite time, really receiving only a certain smaller sum (the difference being discount), and repaying the full sum at the expiration of the allotted time. It is manifest that

when the public is to be borrowed from—that is to say, when a large number of persons are to lend money to some company—the former arrangement would be inconvenient. Many might be prepared to lend money for a time, but not indefinitely; yet it would be most undesirable that the company should have a large number of creditors, any of whom might when they chose demand the return of their money. The plan actually adopted to avoid inconvenience on both sides is nearer the discounting arrangement than the other, though not quite identical with it either. A nominal percentage is offered to the public in many cases; in others the prospect of such and such a percentage; in others a guaranteed percentage, with the possibility of more. Originally the sum paid as one hundred pounds in the way of loan to the company (or one hundred pounds in the company's stock) may be actually one hundred pounds, or may be a sum greater or less offered (as at an auction)—greater if the prospects of the company are regarded as good; less if they are not so highly esteemed. After the original capital of the company—in reality the original loan to the company—has been raised, any part of the capital or loan may pass from one hand to another, but always at such price for the nominal possession of each hundred pounds of stock, or each ten pounds (or whatever may be the unit share), as the prospects of the company are held to justify.

Thus a man who holds a certain amount of stock in any company—be it a nation, or a bank, or a railway, or a trade company—may be considered as for the time

being a man who has nominally lent that sum to the company, and is to receive interest on it at a fixed rate, but who has in reality paid perhaps more or perhaps less than that nominal value because of the higher or lower degree of prosperity and credit possessed by the company. For example, he may hold 1,000*l.* stock in a company paying 5 per cent.; but he may have paid, perhaps, 1,331*l.* for that stock, which is as if he had lent 1,331*l.* for 50*l.* interest per annum—that is really for less than 4 per cent. per annum. Or he may have paid, perhaps, only 817*l.* for the stock, which is as though he had lent 817*l.* for 50*l.* interest per annum, or really for more than 6 per cent. per annum. And in passing, we note that, considering any single company, we see at once how definitely a high rate of interest signifies (as the Duke of Wellington used to tell his officers) low security; for, just as the prosperity and credit of a company rises, so does its stock rise in value, and therefore the rate of interest obtained by purchasers of such stock diminishes, and *vice versâ*.

We note that, according to this method of treating stock in a company, the interest nominally remains unchanged; but the amount to be paid for the nominal sum of 100*l.*, on which 3*l.*, 4*l.*, or 5*l.* (or whatever the nominal rate per cent. may be) is to be paid, varies all the time. It not only varies with actual changes in the prospects of the company, but it varies also as the value of money changes, or as, with the changes in the prospects of other companies, the relative value of the company alters. If, for instance, owing to certain

changes in the value of money, it becomes as easy to secure 5 per cent. per annum on money lent as it had been to secure 4 per cent. when certain stock was bought at a certain price, the value of that stock will evidently be diminished. A buyer, who practically is one proposing to lend money to the company in place of the seller who had already done so, can reasonably expect a better rate of interest when ordinary loans secure a better rate; he, therefore, reasonably expects to pay a smaller sum for the same nominal rate per cent. or per share.

Such being the nature of the stock market, it is obvious that, while investment is a matter which requires much judgment, and should not be entered on without good information from business persons as to the probable stability of the various stocks for sale and purchase, speculation in stocks is utter folly where it is not gross rascality. It is seen to be practically not only *akin* to wagering on the success or failure of a number of persons engaged in business of the nature of which we know nothing, but it is actually this very thing. One might as reasonably go along a street, and, selecting at random any shop, wager that the owner's business will improve during the next week, or that it will fall off, with no surer means of guessing than the look of the shop, as run the eye down the share lists and put cover down on the chance that any particular stock will rise or fall. Nay, wagering on the tradesman's business would be much the safer, for one would see the shop and the goods, one could note the shopman

and his ways, and one might form a shrewd idea as to his probable success or failure. But of the various companies—nations, banks, railways, trade companies, and so forth—in the share list, the cover speculator knows nothing with any certainty, except what is general knowledge and therefore does not help his chance of making a lucky hit. For instance, I know that while Consols are absolutely safe, they will rise or fall as the relations of Great Britain with other nations improve or the reverse; but every one else knows as much. I may know that prospects look favourable or gloomy, but so also will others. I may form a guess as to whether the actual change of value in Consols in any direction will be greater or less than is generally supposed probable; but so soon as I thus pass beyond what is common knowledge, I am as likely to be wrong as to be right. To suppose otherwise is to suppose that where veteran statesmen who know what is actually being done, and the strings which are being actually pulled, can form no sure or trustworthy guess, I can who have no such knowledge. For a cover-speculator, necessarily a simpleton, to buy or sell (nominally) Egyptian, Turkish, or Russian stock, with the idea that he is likely to form a correct opinion where a Gladstone or a Salisbury would be certainly as likely to be wrong as to be right, is preposterous on the face of it. And so also with the railways, banks, and other business companies whose names appear in the share lists. Those who have the best opportunities of knowing the state of affairs in a company have nothing like the con-

fidence in their carefully weighed opinion as to the company's prospects which the cover-speculator has in his fancy that the company's stock must rise or must fall. As the tradesman is content with the amount of chance which enters inevitably into the progress of his business, without wagering on it, so the persons actually engaged in a sound mercantile business on the larger scale are content with the ups and downs which affect the fortunes of all large companies without incurring risk by speculating about them. But fools rush in—— the proverb is something musty.

It may be asked, then, whether money has not been made by speculation, whether it is not a known fact that there are at this moment men of wealth who have made their money entirely by Stock Exchange speculation, never having turned a single honest penny?

Undoubtedly men have become rich in this way, just as men have become rich on the turf. Where otherwise could it be supposed that all the money which the foolish have lost through listening to the wiles of the craftier sort among stockbrokers, or by betting with bookmakers on horses, has gone to? Where tens of thousands of foolish folk are ruined or lose largely, we may be well assured that hundreds of crafty scoundrels have grown rich. These 'drop off gorged' from the schemes which leave those 'flaccid and drained.' The stockbrokers do not get all the money lost by the foolish cover-speculators. In the typical case I cited the stockbrokers made 106*l.* 5*s.* between them, and the lucky and the unlucky speculators lost between them only 56*l.* 5*s.*;

but there I was dealing with the entirely imaginary case of fair speculation. In actual business cover-speculators inevitably fall, in many of their transactions, into the hands of men akin to the bookmakers in turf gambling, who play with cogged dice. Companies are started which have no chance of success as business schemes, but bring money freely into the hands of those who plan them, or being associates of the gang know how to utilise their knowledge. The prices are run up by means familiar to such men, but of which the unfortunate cover-speculator knows nothing. When the swindling scheme has done its work, and all the conspirators have cleared their profits on the rise in the price of shares, the cover-speculator finds himself moved to buy stock in the manifestly promising and prospering concern. To his disgust, but not at first to his alarm, he finds the price of shares at a standstill or even slightly falling. He holds on for the renewed rise which he feels sure—trusting in the judgment he imagines he has in such matters, or in information which he supposes to be trustworthy—will assuredly take place. When the price sinks so as to endanger his deposit, he extends the cover. Presently the bubble explodes, and he finds himself one of the large array of those who have been drained by the rascally promoters.

There are also ways of affecting the price of shares in thoroughly honest concerns by promulgating false rumours ; and many a poor wretch, who has complained of fortune frowning when he has seen cover after cover impounded through the fall of shares when he had

expected a rise, and *vice versâ*, has been the victim of anything but fortune's assaults; his money has been as deliberately stolen as if his pocket had been picked.

So certain is eventual loss to the cover-speculator that I would endorse the saying of an esteemed friend of mine, a merchant in St. Joseph, Missouri, who when a young man boasted of gaining a large sum by dealing in 'corners in grain' (a system precisely similar to the cover system, only the varying prices of particular kinds of grain, instead of the prices of particular stocks, decide the question of loss or gain), told the lucky gambler that the very best thing he could do with his winnings was to fling them into the Missouri.

In fine, no one has any but the minutest chance of failing to lose largely by cover-speculation—unless he is prepared to speculate with such knowledge as would make every transaction a villainy.

FALLACIES AND COINCIDENCES.

Every one is familiar with the occasional occurrence of coincidences, so strange—considered abstractly—that it appears difficult to regard them as due to mere casualty. The mind is dwelling on some person or event, and suddenly a circumstance happens which is associated in some altogether unexpected, and as it were improbable, manner with that person or event. A scheme has been devised which can only fail if some utterly unlikely series of events should occur, and precisely those events take place. Sometimes a coincidence is utterly trivial, yet attracts attention by the singular improbability of the observed events. We are thinking of some circumstance, let us say, in which two or three persons are concerned, and the first book or paper we turn to shows, in the very first line we look at, the names of those very persons, though really relating to others in no way connected with them; and so on, with many other kinds of coincidence, equally trivial and equally singular. Yet again, there are other coincidences which are rendered striking by their frequent recurrence. It is to such recurring coincidences that common superstitions owe their origin, while the special superstitions

thus arising (that is, superstitions entertained by individuals) are innumerable. It is lucky to do this, unlucky to do that, say those who believe in common superstitions; and they can always cite many coincidences in favour of their opinion. But it is amazing how common are the private superstitions entertained by many who smile at the superstitions of the ignorant: we must suppose that all such superstitions have been based upon observed coincidences. Again, there are tricks or habits which have obviously had their origin in private superstitions. Dr. Johnson may not have believed that some misfortune would happen to him if he failed to place his hand on every post which he passed along a certain route; he would certainly not have maintained such an opinion publicly: yet in the first instance that habit of his must have had its origin in some observed coincidences; and when once a habit of the sort is associated with the idea of good luck, even the strongest minds have been found unready to shake off the superstition.[1]

[1] Here, for instance, is an account given by one keen card-player of another who was as keen, or keener. 'He was very particular about cutting the cards; he always insisted on the pack being perfectly square before he would cut, and that they should be placed in a convenient position. There is an old adage that a slovenly cut is good for the dealer, but whether there is truth in the statement we know not. He was superstitious to a degree that was astonishing.' (It must be a rather startling superstition that would seem astonishing to a man who could gravely ask whether there is any truth in the preposterous adage just quoted.) 'We are not aware that any one has ever attempted to solve the problem why so many great minds' (among card-players, fighting men, and men who have to work much at odds with fortune) 'are superstitious. This is not the time or place to attempt that solution. We record the fact.'

It is to be noticed, indeed, that many who reject the idea that the ordinary superstitions have any real significance, are nevertheless unwilling to run directly counter to them. Thus, a man shall be altogether sceptical as to the evil effects which follow, according to a common superstition, from passing under a ladder; he may be perfectly satisfied that the proper reason for not passing under a ladder is the possibility of its falling, or of something falling from it: yet he will not pass under a ladder, even though it is well secured, and obviously carries nothing which can fall upon him. So with the old superstition, that a broken mirror brings seven years of sorrow, which, according to some, dates from the time when a mirror was so costly as to represent seven years' savings—there are those who despise the superstition who would yet be unwilling to tempt fate (as they put it) by wilfully breaking even the most worthless old looking-glass. A story is not unfrequently quoted in defence of such caution. Every one knows that sailors consider it unlucky for a ship to sail on a Friday. A person, anxious to destroy this superstition, had a ship's keel laid on a Friday, the ship launched on a Friday, her masts taken

He believed in dress having something to do with luck, and if the luck followed him, he would wear the same dress, whether it was adapted to the weather or not. He believed in cards and seats. He objected to any one making a remark about his luck. He had the strongest objection to our backing him, because of our bad luck, and we have often had to refrain from taking odds, because of this fad. He was distressed beyond measure if any one touched his counters. His constant system of shuffling the cards was at times an annoyance.' This was a great card-player!

in from the sheer-hulk on a Friday, the cargo shipped on a Friday; he found (heaven knows how, but so the story runs) a Captain Friday to command her; and lastly, she sailed on a Friday. But the superstition was not destroyed, for the ship never returned to port, nor was the manner of her destruction known. Other instances of the kind might be cited. Thus a feeling is entertained by many persons not otherwise superstitions, that bad luck will follow any wilful attempt to run counter to a superstition.

It is somewhat singular that attempts to correct even the more degrading forms of superstition have often been as unsuccessful as those attempts which may perhaps not unfairly be called tempting fate. Let me be understood. To refer to the example already given, it is a manifest absurdity to suppose that the sailing of a ship on a Friday is unfortunate; and it would be a piece of egregious folly to consider such a superstition when one has occasion to take a journey. But the case is different when any one undertakes to *prove* that the superstition is an absurdity; simply because he must assume in the first instance that he will succeed, a result which cannot be certain; and such confidence, apart from all question of superstition, is a mistake. In fact, a person so acting errs in the very same way as those whom he wishes to correct; *they* refrain from a certain act because of a blind fear of bad luck, and *he* proceeds to the act with an equally blind belief in good luck.

But one cannot recognise the same objection in the

case of a person who tries to correct some superstition by actions not involving any tempting of fortune. Yet it has not unfrequently happened that such actions have resulted in confirming the superstition. The following instance may be cited. An old woman came to Flamsteed, the first Astronomer-Royal, to ask him whereabouts a certain bundle of linen might be, which she had lost. Flamsteed determined to show the folly of that belief in astrology which had led her to Greenwich Observatory (under some misapprehension as to the duties of an Astronomer-Royal). He 'drew a circle, put a square into it, and gravely pointed out a ditch, near her cottage, in which he said it would be found.' He then waited until she should come back disappointed, and in a fit frame of mind to receive the rebuke he intended for her; but 'she came back in great delight, with the bundle in her hand, found in the very place.'

In connection with this story, though bearing rather on over-hasty scientific theorising than on ordinary superstitions, I quote the following story from De Morgan's 'Budget of Paradoxes':—'The late Baron Zach received a letter from Pons, a successful finder of comets, complaining that for a certain period he had found no comets, though he had searched diligently. Zach, a man of much sly humour, told him that no spots had been seen on the sun for about the same time—which was true—and assured him that when the spots came back the comets would come with them. Some time after he got a letter from Pons, who informed him

with great satisfaction that he was quite right; that very large spots had appeared on the sun, and that he had found a comet shortly after. I have the story in Zach's handwriting. It would mend the story exceedingly if some day a real relation should be established between comets and solar spots. Of late years good reason has been shown for advancing a connection between these spots and the earth's magnetism. If the two things had been put to Zach he would probably have chosen the comets. Here is a hint for a paradox: the solar spots are the dead comets, which have parted with their light and heat to feed the sun, as was once suggested. I should not wonder if I were too late, and the thing had been actually maintained.' De Morgan was not far wrong. Something very like his paradox was advocated, before the Royal Astronomical Society, by Commander Ashe, of Canada, earlier we believe than the date of De Morgan's remarks. I happen to have striking evidence in favour of De Morgan's opinion about the view which Zach would probably have formed of the theory which connects sun-spots and the earth's magnetism. When the theory was as yet quite new, I referred to it in a company of Cambridge men, mostly high mathematicians, and it was received at first as an excellent joke, and welcomed with laughter. It need hardly be said, however, that when the nature of the evidence was stated, the matter assumed another aspect. Yet it may be mentioned, in passing, that there are those who maintain that, after all, this theory is untrue,

the evidence on which it rests being due only to certain strange coincidences.

In many instances, indeed, considerable care is required to determine whether real association or mere casual coincidence is in question. It is surprising how, in some cases, an association can be traced between events seemingly in no way connected. One is reminded of certain cases of derivation. Ninety-nine persons out of a hundred, for instance, would laugh at the notion that the words 'hand' and 'prize' are connected; yet the connection is seen clearly enough when 'prize' is traced back to 'prehendo,' with the root 'hend' obviously related to 'hand,' 'hound,' and so on. Equally absurd at a first view is the old joke that the Goodwin Sands were due to the building of a certain church; yet if moneys which had been devoted to the annual removal of the gathering sand were employed to defray the cost of the church, mischief, afterwards irreparable, might very well have been occasioned. Even the explanation of certain mischances as due to the circumstance that 'there was no weathercock at Kiloe,' may admit of a not quite unreasonable interpretation. I leave this as an exercise for the ingenious reader.

But when we have undoubted cases of coincidence, without the possibility of any real association (setting the supernatural aside), we have a problem of some interest to deal with. To explain them as due to some special miraculous intervention may be satisfactory to many minds, in certain cases; but in others it is impossible to conceive that the matter has seemed

worthy of a miracle. Even viewing the question in its bearing on religious ideas, there are cases where it seems far more mischievous (as bringing ridicule on the very conception of the miraculous) to believe in supernatural intervention, than to reject such an explanation on the score of antecedent improbability. Horace's rule, '*Nec deus intersit nisi dignus vindice nodus*,' remains sound when we write '*Deus*' for '*deus*.'

Now there have been cases so remarkable, yet so obviously unworthy of supernatural intervention, that we are perplexed to find any reasonable explanation of the matter. The following, adduced by De Morgan, will, I have no doubt, recall corresponding cases in the experience of readers of these lines :—'In the summer of 1865,' he says, 'I made myself first acquainted with the tales of Nathaniel Hawthorne, and the first I read was about the siege of Boston in the War of Independence. I could not make it out: everybody seemed to have got into somebody else's place. I was beginning the second tale when a parcel arrived: it was a lot of odd pamphlets and other rubbish, as he called it, sent by a friend who had lately sold his books, had not thought it worth while to send these things for sale, but thought I might like to look at them, and possibly keep some. The first thing I looked at was a sheet, which, being opened, displayed "A plan of Boston and its environs, showing the true situation of his Majesty's army, and also that of the rebels, drawn by an engineer, at Boston, October 1775."

Such detailed plans of current sieges being then uncommon, it is explained that "The principal part of this plan was surveyed by Richard Williams, Lieutenant, at Boston; and sent over by the son of a nobleman to his father in town, by whose permission it was published." I immediately saw that my confusion arose from my supposing that the king's troops were besieging the rebels, when it was just the other way' (a mistake, by the way, which does not suggest that the narrative was particularly lucid).

Another instance cited by De Morgan is yet more remarkable, though it is not nearly so strange as a circumstance which I shall relate afterwards:—'In August, 1861,' he says, 'M. Senarmont, of the French Institute, wrote to me to the effect that Fresnel had sent to England in, or shortly after, 1824, a paper for translation and insertion in the "European Review" which shortly after expired. The question was what had become of the paper. I examined the "Review" at the Museum, found no trace of the paper, and wrote back to that effect, at the Museum, adding that everything now depended on ascertaining the name of the editor, and tracing his papers: of this I thought there was no chance. I posted the letter on my way home, at a post-office in the Hampstead Road, at the junction with Edward Street, on the opposite side of which is a bookstall. Lounging for a moment over the exposed books, *sicut meus est mos*, I saw within a few moments of the posting of the letter a little catchpenny book of anecdotes of Macaulay, which I bought, and ran over for

a minute. My eye was soon caught by this sentence:—"One of the young fellows immediately wrote to the Editor (Mr. Walker) of the 'European Review.'" I thus got the clue by which I ascertained that there was no chance of recovering Fresnel's papers. Of the mention of current Reviews not one in a thousand names the editor.' It will be noticed that there was a double coincidence in this case. It was sufficiently remarkable that the first mention of a review, after the difficulty had been recognised, should relate to the 'European,' and give the name of the editor; but it was even more remarkable that the occurrence should be timed so strangely as was actually the case.

But the circumstance I am now to relate seems to me to surpass in strangeness all the coincidences I have ever heard of. It relates to a matter of considerable interest apart from the coincidence.

When Dr. Thomas Young was endeavouring to interpret the inscription of the famous Rosetta Stone, Mr. Grey (afterwards Sir George Francis Grey) was led on his return from Egypt to place in Young's hands some of the most valuable fruits of his researches among the relics of Egyptian art, including several fine specimens of writing on papyrus, which he had purchased from an Arab at Thebes, in 1820. Before these had reached Young, a man named Casati had arrived in Paris, bringing with him from Egypt a parcel of Egyptian manuscripts, among which Champollion observed one which bore in its preamble some resemblance to the text of the Rosetta Stone. This

discovery attracted much attention; and Dr. Young having procured a copy of the papyrus, attempted to decipher and translate it. He had made some progress with the work when Mr. Grey gave him the new papyri. 'These,' says Dr. Young, 'contained several fine specimens of writing and drawing on papyrus; they were chiefly in hieroglyphics and of a mythological nature; but two which he had before described to me, as particularly deserving attention, and which were brought, through his judicious precautions, in excellent preservation, both contained some Greek characters, written apparently in a pretty legible hand. That which was most intelligible had appeared at first sight to contain some words relating to the service of the Christian Church.' Passing thence to speak of Casati's papyrus, Dr. Young remarks that it was the first in which any intelligible characters of the enchorial form had been discovered among the many manuscripts and inscriptions which had been examined, and it 'furnished M. Champollion with a name which materially advanced the steps leading him to his very important extension of the hieroglyphical alphabet. He had mentioned to me, in conversation, the names of Apollonius, Antiochus, and Antigonus, as occurring among the witnesses; and I easily recognised the groups which he had deciphered; although, instead of *Antiochus*, I read Antimachus; and I did not recollect at the time that he had omitted the m.'

Now comes the strange part of the story.

'In the evening of the day that Mr. Grey had

brought me his manuscripts,' proceeds Dr. Young (whose English, by the way, is in places slightly questionable), 'I proceeded impatiently to examine that which was in Greek only; and I could scarcely believe that I was awake and in my sober senses, when I observed among the names of the witnesses *Antimachus Antigenis* (*sic*); and a few lines farther back, *Portis Apollonii*; although the last word could not have been very easily deciphered without the assistance of the conjecture, which immediately occurred to me, that this manuscript might perhaps be a translation of the enchorial manuscript of Casati. I found that its beginning was, "A copy of an Egyptian writing"; and I proceeded to ascertain that there were the same number of names intervening between the Greek and the Egyptian signatures that I had identified, and that the same number followed the last of them. The whole number of witnesses was sixteen in each. . . . I could not therefore but conclude,' proceeds Dr. Young, after dwelling on other points equally demonstrative of the identity of the Greek and enchorial inscriptions, 'that a most extraordinary chance had brought into my possession a document which was not very likely, in the first place, ever to have existed, still less to have been preserved uninjured, for my information, through a period of near two thousand years; but that this very extraordinary translation should have been brought safely to Europe, to England, and to me, at the very moment when it was most of all desirable to me to possess it, as the

illustration of an original which I was then studying, but without any other reasonable hope of comprehending it; this combination would, in other times, have been considered as affording ample evidence of my having become an Egyptian sorcerer.' The surprising effect of the coincidence is increased when the contents of this Egyptian manuscript are described. 'It relates to the sale, not of a house or a field, but of a portion of the collections and offerings made from time to time on account or for the benefit of a certain number of mummies of persons described at length in very bad Greek, with their children and all their households.'

The history of astronomy has in quite recent times afforded a very remarkable instance of repeated coincidences. I refer to the researches by which the theory has been established, that meteors and comets are so far associated that meteor systems travel in the tracks of comets. It will readily be seen from the following statements, all of which may be implicitly relied upon, that the demonstration of this theory must be regarded as partly due to singular good fortune:

There are two very remarkable meteor systems—the system which produces the November shooting-stars, or *Leonides*, and that which produces the August shooting-stars, or *Perseides*. It chanced that the year 1866 was the time when a great display of November meteors was expected by astronomers. Hence, in the years 1865 and 1866 considerable attention was directed to the whole subject of shooting-stars. More-

over, so many astronomers watched the display of 1866, that very exact information was for the first time obtained as to the apparent track of these meteors. It is necessary to mention that such information was *essential* to success in the main inquiry. Now it had chanced that in 1862 a fine comet had been seen, whose path approached the earth's path very closely indeed. This led the Italian astronomer Schiaparelli to inquire whether there might not be some connection between this comet and the August shooting-stars, which cross the earth's path at the same place. He was able, by comparing the path of the comet and the apparent paths of the meteors, to render this opinion highly probable. Then came inquiries into the real paths of the November meteors, these inquiries being rendered just practicable by several coincidences, as —(1) the exact observations just mentioned; (2) the existence of certain old accounts of the meteor shower; (3) the wonderful mastery obtained by Professor Adams over all problems of perturbation (for the whole question depended on the way in which the November meteors had been perturbed); and (4) the existence of a half-forgotten treatise by Gauss, supplying formulæ which reduced Adams' labour by one-half. The path having been determined (by Adams alone, I take this opportunity of insisting),[1] the

[1] Leverrier, Schiaparelli, and others calculated the path on the assumption that the occurrence of displays three times per century implies a periodic circulation around the sun in about thirty-three years and a quarter; but Adams alone proved that this period, and no other, must be that of the November meteors.

whole question rested on the recognition of a comet travelling in the same path. If such a comet were found, Schiaparelli's case was made out. If not, then, though the evidence might be convincing to mathematicians well grounded in the theory of probabilities, yet it was all but certain that Schiaparelli's theory would presently sink into oblivion. Now there are probably hundreds of comets which have a period of thirty-three and a quarter years, but very few are known—only three certainly—and one of these *had only just been discovered* when Adams' results were announced. The odds were enormous against the required comet being known, and yet greater against its having been so well watched that its true path had been ascertained. Yet the comet which had been discovered in that very year 1866—the comet called Tempel's, or I. 1866—was the very comet required to establish Schiaparelli's theory. *There* was the path of the meteors assigned by Adams, and the path of the comet had been already calculated by Tempel before Adams' result had been announced; and these two paths were found to be to all intents and purposes (with an accuracy far exceeding indeed the requirements of the case) *identical*.

To the remarkable coincidences here noted, coincidences rendered so much the more remarkable by the fact that the August comet is now known to return only twice in three centuries, while the November comet returns only thrice per century, may be added these:

The comet of 1862 was observed, telescopically, by Sir John Herschel under remarkably favourable circumstances. 'It passed us closely and swiftly,' says Herschel, 'swelling into importance, and dying away with unusual rapidity. The phenomena exhibited by its nucleus and head were on this account peculiarly interesting and instructive, *it being only on very rare occasions* that a comet can be closely inspected at the very crisis of its fate, so that we can witness the actual effect of the sun's rays on it.' (This was written long before Schiaparelli's theory had attracted notice.) This comet was also the last observed and studied by Sir John Herschel. The November comet, again, was the *first comet ever analysed with the spectroscope.*

It will be remarked, perhaps, that where coincidences so remarkable as these are seen to be possible, it may be questionable whether the theory itself, which is based on the coincidence of certain paths, can be accepted as trustworthy. It is to be noticed that, whether this be so or not, the surprising nature of the coincidence is in no way affected; it would be as remarkable (at least) that so many events should concur to establish a false as to establish a true theory. This noted, we may admit that in this case, as in many others, the evidence for a scientific theory amounts in reality only to extreme probability. However, it is to be noticed that the probability for the theory belongs to a higher *order* than the probability against those observed coincidences which rendered the demonstration of the theory possible. The odds were thousands

to one, perhaps, against the occurrence of these coincidences: but they are millions to one against the coincidence of the paths as well of the November as of the August meteors with the paths of known comets, by mere accident.

It may possibly be considered that the circumstances of the two last cases are not altogether such as to assure us that special intervention was not in question in each instance. Indeed, though astronomers have not recognised anything supernatural in the series of events which led to the recognition of the association between meteors and comets, some students of archæology have been disposed to regard the events narrated by Dr. Young as strictly providential dispensations. 'It seems to the reflective mind,' says the author of the 'Ruins of Sacred and Historic Lands,' 'that the appointed time had at length arrived when the secrets of Egyptian history were at length to be revealed, and to cast their reflective light on the darker pages of sacred and profane history. . . . The incident in the labours of Dr. Young seems so surprising that it might be deemed providential, if not miraculous.' The same will scarcely be thought of such events (and their name is legion) as De Morgan has recorded; since it requires a considerable stretch of imagination to conceive that either the discovery of the name of a certain editor, or the removal of De Morgan's difficulties respecting the siege of Boston, was a *nodus* worthy of miraculous interposition.

For absolute triviality, however, combined with sin-

gularity of coincidence, a circumstance which occurred to me several years ago appears unsurpassable. I was raising a tumbler in such a way that at the moment it was a few inches above my mouth ; but whether to examine its substance against the light, or for what particular purpose, has escaped my recollection. Be that as it may, the tumbler slipped from my fingers and fell so that the edge struck against one of my lower teeth. The fall was just enough to have broken the tumbler (at least, against a sharp object like a tooth), and I expected to have my mouth unpleasantly filled with glass fragments and perhaps seriously cut. However, though there was a sharp blow, the glass remained unbroken. On examining it, I found that a large drop of wax had fallen on the edge at the very spot where it had struck my tooth, an indentation being left by the tooth. Doubtless the softening of the shock by the interposition of the wax had just saved the glass from fracture. In any case, however, the surprising nature of the coincidence is not affected. On considering the matter it will be seen how enormous were the antecedent odds against the observed event. It is not an usual thing for a tumbler to slip in such a way : it has not at any other time happened to me, and probably not a single reader of these lines can recall such an occurrence either in his own experience or that of others. Then it very seldom happens, I suppose, that a drop of wax falls on the edge of a tumbler and there remains unnoticed. That two events so unusual should be coincident, and that the

very spot where the glass struck the tooth should be the place where the wax had fallen, certainly seems most surprising. In fact, it is only the utter triviality of the whole occurrence which renders it credible; it is just one of those events which no one would think of inventing. Whether credible or not, it happened. As De Morgan says of the coincidences he relates, so can I say for the above (equally important) circumstance, 'I can solemnly vouch for its literal truth.' Yet it would be preposterous to say that there was anything providential in such an occurrence. Swift, in his 'Tale of a Tub,' has indicated in forcible terms the absurdity of recognising miraculous interventions in such cases; but should it appear to some of my readers that, trivial though the event was, I should have recognised the hand of Providence in it, I would remark that it requires some degree of self-conceit to regard oneself as the subject of the special intervention of Providence, and moreover that Providence might have contrived the escape in less complicated sort by simply so arranging matters that the glass had not fallen at all. So, at least, it appears to me.

There arises, in certain cases, the question whether coincidences may not appear so surprising as to justify the assumption that they are due to a real though undiscerned association between the coinciding events. This, of course, is the very basis of the scientific method; and it is well to notice how far this method may sometimes be unsafe. If remarkable coincidences

can occur when there is no real connection—as we have seen to be the case—caution must be required in recognising coincidence as demonstrative of association.

The rule of science in all such cases is simply to inquire whether there can possibly be any relation of cause and effect in such cases. When a housemaid says, for instance, that putting the poker across a fire makes the fire burn up, the student of physical laws is able at once to see that the supposed influence is antecedently most improbable. Here in a grate are certain more or less combustible materials, and certain quantities of matter already burning; combustion is going on, though indifferently; the air is nourishing this slowly burning fire, but inefficiently; on the whole, it seems likely that the fire will go out. In what way shall I do any good if I stick a rod of iron from the fender across the top bar? I thus add a certain quantity of cold metal to the space across which the air has to come to the fire. Do I increase the draught? On the contrary, so far as I produce any effect at all on the draught, I must diminish it. For the draught depends in the main on the diminished density of the warmed air in the neighbourhood of the fire, and the cold metal must to some degree increase the density of this air by cooling it. The effect may be very slight; but such as it is, it is unfavourable. But I was once told by a correspondent that whether theoretically the poker should make the fire burn up or not, as a matter of fact it does. Repeatedly he had tried the experiment, and after ex-

hausting in vain every art he possessed to make the fire burn up, he found that the poker when put across the top bar immediately, or almost immediately, produced the desired result. Science is bound to listen to evidence of this kind, for science deals with phenomena, and even when phenomena seem to point to something which appears utterly incredible, science has to inquire into the matter. Well, in this case, what are the facts? Some one tells us that he has repeatedly tried in vain to make a fire burn up, but when he put the poker across it, the fire presently became clear and bright. Multitudes of contrary cases might no doubt be cited, but let us suppose that none could. Are we therefore to infer that in these cases the poker drew the fire up? A new law of nature would be indicated if this were so; and a new law of nature is worth learning. But when due inquiry is made, it appears that there is no such law—as unfortunately we might have expected. Our correspondent, who found that when he put the poker across the fire it drew up, is unquestionably but an unskilful fireman. He puts on coals, and pokes and stirs the fire, unconscious of the fact that this is just the way to put a fire out. When the fire is all but hopelessly reduced by his unskilful measures, he puts the poker across the top bar. According to old-fashioned superstitions, he makes the sign of the cross across the fire-place, and the fire, in which until now there seemed to have been some evil spirit (that is what people mean when they say 'the devil's in the fire'), is purified from the unclean presence and

begins to burn up. That would have been the old-fashioned interpretation of the change; but science takes another view of the matter. It sees reason to believe that the change took place simply because the disturbance to which the fire had before been exposed was bad for it. Putting the poker across the top bar meant letting the fire alone, and giving it a chance to burn up.

Singularly enough, I had occasion, when the last sentence was just finished, to leave my study. When I came back, an hour later, I found that my fire, which in the meantime must very nearly have gone out, had been recoaled—and the housemaid, or whoever had attended to it, had, after the fashion of her tribe, put the poker across the top bar. The fire was not burning very brightly—on the contrary, it seemed inclined to go out. Yet, rashly daring, I put the poker down—from scientific principles I object to seeing bright metal smoked and dulled—and went on with my work, intending, if the fire went out, to call some one in to light it again. However, it so chanced that after the poker was put down, the fire began to burn pretty brightly, and as I write there is every promise of a good fire. Am I to infer that taking the poker from across the top bar made the fire burn up? Of course, the real fact was, that when the fire seemed dull it was really making steady progress, and whether I had taken down the poker, or supplemented its salutary action by putting another poker across the top bar, would not have made one particle of difference.

That our domestic servants should consider the poker across the top bar a specific for making a dull fire burn up is very natural. Their manner of treating fires is unscientific in the extreme. A Cambridge Fellow, who knew very little about the fair sex, except what he might gather from the ways of 'bed-makers' and his recollections, perhaps, of domestic servants at home, used to define woman as 'an inferior animal, not understanding logic, and poking a fire from the top.' Most servants do this. They also have two utterly erroneous ideas about making up a low fire: first, that the more fuel is put on the better; secondly, that after putting coal on it is desirable to stir the fire. As a matter of fact, when a fire is low, the addition of fuel will often put it out altogether, and the addition of much fuel is almost certain to do so; and in every case the time to stir the fire (when low) is before coals are put on, not after. Generally it is well, when a fire is low, to stir it deftly, so as to bring together the well-burning parts, and then to wait a little, till they begin to glow more brightly; then a few coals may be put on, and after awhile the fire may again be stirred and some more coals put on it. When a low fire has been unwisely treated by being coaled too freely, and the fresh fuel uselessly stirred, it is generally the case that the only chance for the fire is leaving it alone. Susan does this when she puts the poker across the top bar, and unconsciously she retains the old superstition that, by thus making the sign of the cross over the fire, she sends away the evil beings, sprites, or

whatever they may have been, which were extinguishing it.

That letting the sun shine on a fire puts it out is not, like the other (in its real origin, at any rate), a superstition, but simply an illusion. A correspondent wrote to me that it is believed in by nine persons out of ten; but in this it is like all other wrong beliefs. Scientific methods of inquiry and reasoning are followed by fewer than ten in a hundred; and although nowadays the views of science are accepted more widely than in olden times, this is simply because science has shown its power by material conquests.[1]

Not to take any more scientific instances, of which perhaps I have already said enough, let us consider the case of presentiments of death or misfortune. Here, in the first place, the coincidences which have been recorded are not so remarkable as might at first sight appear, simply because such presentiments are very common indeed. A certain not unusual condition of health, the pressure of not uncommon difficulties or dangers, depression arising from atmospheric and other

[1] I do not think that my friend Professor Tomlinson's experiments on the burning of candles in sunlight and in the dark would be regarded by all as decisively showing that sunlight does not interfere with combustion, though, rightly apprehended, they go near to prove this. But *à priori* considerations show conclusively that though by warming the air around a fire the sun's rays may, in some slight degree (after a considerable time), affect the progress of combustion, they cannot possibly put the fire out in the sense in which they are commonly supposed to do so; in fact, a fire would probably burn somewhat longer in a room well warmed by a summer sun than in a room from which the solar rays were excluded. (The difference would be very slight.)

causes, many circumstances, in fact, may suggest (and do notoriously suggest) such presentiments. That some presentiments out of very many thus arising should be fulfilled is not to be regarded as surprising—on the contrary, the reverse would be very remarkable. But again a presentiment may be founded on facts, known to the person concerned, which may fully justify the presentiment. 'Sometimes,' says De Morgan on this point, 'there is no mystery to those who have the clue.' He cites instances. 'In the "Gentleman's Magazine" (vol. 80, part 2, p. 33) we read, the subject being presentiment of death, as follows:—"In 1718, to come nearer the recollection of survivors, at the taking of Pondicherry, Captain John Fletcher, Captain De Morgan"' (De Morgan's grandfather) '"and Lieutenant Bosanquet each distinctly foretold his own death on the morning of his fate." I have no doubt of all three; and I knew it of my grandfather long before I read the above passage. He saw that the battery he commanded was unduly exposed—I think by the sap running through the fort when produced.[1] He represented this to the engineer officers, and to the commander-in-chief; the engineers denied the truth of the statement, the commander believed them, my grandfather quietly observed that he must make his will, and the French fulfilled the prediction. His will bore date the day of his death; and I always thought it

[1] De Morgan writes somewhat inexactly here for a mathematician. The sap did not run through the fort, but the direction of the sap so ran.

more remarkable than the fulfilment of his prophecy that a soldier should not consider any danger short of one like the above sufficient reason to make his will. I suppose,' proceeds De Morgan, 'the other officers were similarly posted. I am told that military men very often defer making their wills until just before an action; but to face the ordinary risks intestate, and to wait until speedy death must be the all but certain consequence of a stupid mistake, is carrying the principle very far.'

As to the fulfilment of dreams and omens, it is to be noticed that many of the stories bearing on this subject fail in showing that the dream was fully described *before* the event occurred which appeared to fulfil the dream. It is not unlikely that if this had been done, the fulfilment, in many cases, would not have appeared quite so remarkable as in the actual narrative. Without imputing untruth to the dreamer, we may nevertheless—merely by considering what is known as to ordinary testimony—believe that the occurrences of the dream have been somewhat modified after the event. I do not doubt that if every person who had a dream leaving a strong impression on the mind, were at once to record all the circumstances of the dream, very striking instances of fulfilment would occur before long; but at present, certainly, nine-tenths of the remarkable stories about dreams fail in the point I have referred to.

The great objection, however, to the theory that certain dreams have been intended to foreshadow real

events, is the circumstance that the instances of fulfilment are related, while the instances of non-fulfilment are forgotten. It is known that instances of the latter sort are very numerous, but what proportion they bear to instances of the former sort, is unknown; and while this is the case, it is impossible to form any sound opinion on the subject, so far as actual evidence is concerned. It must be remembered that in this case we are not dealing with a theory which will be disposed of if one undoubted negative instance be adduced. It is very difficult to draw the line between dreams of an impressive nature—such dreams as we might conceive to be sent by way of warning—and dreams not specially calculated to attract the dreamer's attention. A dream which appeared impressive when it occurred but was not fulfilled by the event, would be readily regarded, even by the dreamer himself, as not intended to convey any warning as to the future. The only way to form a just opinion would be to record each dream of an impressive nature, immediately after its occurrence, and to compare the number of cases in which such dreams are fulfilled with the number in which there is no fulfilment. Let us suppose that a certain class of dreams were selected for this purpose. Thus, let a society be formed, every member of which undertakes that whenever on the night preceding a journey he dreams of misfortune on the route, he will record his dream, with his ideas as to its impressiveness, before starting on his journey. A great number of such cases would soon be collected,

and we may be sure that there would be several striking fulfilments, and probably two or three highly remarkable cases of the sort; but for my own part, I strongly entertain the opinion that the percentage of fulfilments would correspond very closely with the percentage due to the common risks of travelling, with or without premonitory dreams. This could readily be tested, if the members of the society agreed to note every occasion on which they travelled: it would be found, I suspect, that the dreamers gained little by their warnings. Suppose, for instance, that ten thousand journeys of all sorts were undertaken by the members of the society in the course of ten years, and that a hundred of these journeys (one per cent., that is) were unfortunate; then, if one-tenth of the journeys (a thousand in all) were preceded by warning dreams, I conceive that about ten of these warnings (or one per cent.) would be fulfilled. If more were fulfilled there would appear, so far as the evidence went, to be a balance of meaning in the warnings; if fewer, it would appear that warning dreams were to some slight degree to be interpreted by the rule of contraries; but if about the proper average number of ill-omened voyages turned out unfortunately, it would follow that warning dreams had no significance or value whatever: and this is precisely the result I should expect.

Similar reasoning, and perhaps a similar method, might be applied to cases where the death of a person has been seemingly communicated to a friend or relative at a distance, whether in a dream or vision,

or in some other way at the very instant of its occurrence. It is not, however, by any means so clear that in such instances we may not have to deal with phenomena admitting of physical interpretation. This is suggested, in fact, by the application of considerations resembling those which lead to the rejection of the belief that dreams give warning against dangers. Dreams of death may indeed be sufficiently common, and but little stress could be laid, therefore, on the fulfilment of several or even of many such dreams. But visions of the absent are not common phenomena. That state of the health which occasions the appearance of visions is unusual; and if some of the stories of death-warnings are to be believed, visions of the absent have appeared to persons in good health. But setting aside the question of health, visions are unusual phenomena. Hence, if any considerable proportion of those narratives be true, which relate how a person has at the moment of his death appeared in a vision to some friend at a distance, we must recognise the possibility, at least, that under certain conditions mind may act on mind independently of distance. The *à priori* objections to this belief are, indeed, very serious, but *à priori* reasoning does not amount to demonstration. We do not *know* that even when under ordinary circumstances we think of an absent friend, his mind may not respond in some degree to our thoughts, or else that our thoughts may not be a response to thoughts in his mind. It is certain that such a law of thought might exist and remain undetected—it would

indeed be scarcely detectable. At any rate, we know too little respecting the mind to be certain that no such law exists. If it exists, then it is quite conceivable that the action of the mind in the hour of death might raise a vision in the mind of another.

I shall venture to quote here an old but well-authenticated story, as given by Mr. Owen in his 'Debatable Land between this World and the Next,' leaving to my readers the inquiry whether probabilities are more in favour of the theory that (1) the story is untrue, or (2) the event related was only a remarkable coincidence between a certain event and a certain cerebral phenomenon, in reality no way associated with it, or (3) that there was a real association physically explicable, or (4) that the event was supernatural. Lord Erskine related to Lady Morgan — herself a perfect sceptic—(I wish, all the same, that the story came direct from Erskine) the following personal narrative:—'On arriving at Edinburgh one morning, after a considerable absence from Scotland, he met in the street his father's old butler, looking very pale and wan. He asked him what brought him to Edinburgh. The butler replied, "To meet your honour, and solicit your interference with my lord to recover a sum due to me, which the steward at the last settlement did not pay." Lord Erskine then told the butler to step with him into a bookseller's shop close by, but on turning round again he was not to be seen. Puzzled at this he found out the man's wife, who lived in Edinburgh, when he learnt for the first time that the butler was

dead, and that he had told his wife, on his death-bed, that the steward had wronged him of some money, and that when Master Tom returned he would see her righted. This Lord Erskine promised to do, and shortly afterwards kept his promise.' Lady Morgan then says, 'Either Lord Erskine did or did not believe this strange story: if he did, what a strange aberration of intellect! if he did not, what a stranger aberration from truth! My opinion is that he *did* believe it.' Mr. Owen deals with the hypothesis that aberration of intellect was in question, and gives several excellent reasons for rejecting that hypothesis; and he arrives at the conclusion that the butler's phantom had really appeared after his death. 'The natural inference from the facts, if they are admitted, is,' he says, 'that under certain circumstances, which as yet we may be unable to define, those over whom the death-change has passed, still interested in the concerns of earth, may for a time at least retain the power of occasional interference in these concerns; for example, in an effort to right injustice done.' He thus adopts what, for want of a better word, may be called the supernatural interpretation. But it does not appear from the narrative (assuming it to be true) that the butler was dead at the moment when Erskine saw the vision and heard the words. If this moment preceded the moment of the butler's death, the story falls into the category of those which seem explicable by the theory of brain-waves. I express no opinion.

I had intended to pass to the consideration of those

appearances which have been regarded as ghosts of departed persons, and to the study of some other matters which either are or may be referred to coincidences and superstitions. But my space is exhausted. Perhaps I may hereafter have an opportunity of returning to the subject—not to dogmatise upon it, nor to undertake to explain away the difficulties which surround it, but to indicate the considerations which, as it appears to me, should be applied to the investigation of such matters by those who wish to give a reason for the belief that is in them.

At present I must be content with indicating the general interpretation of coincidences which appear very remarkable, but which nevertheless cannot be reasonably referred to special interpositions of Providence. The fact really is that occasions are continually occurring where coincidences of the sort are *possible*, though improbable. Now the improbability in any particular case would be a reasonable ground for expecting that in that case no coincidence would occur. But the matter is reversed when a great multitude of cases are in question. The probable result then is that there *will* be coincidences. This may easily be illustrated by reference to a question of ordinary probabilities. Suppose there is a lottery with a thousand tickets and but one prize. Then it is exceedingly unlikely that any particular ticket-holder will obtain the prize—the odds are, in fact, 999 to 1 against him. But suppose he had one ticket in each of a million different lotteries all giving the same

chance of success. Then it would not be surprising for him to draw a prize; on the contrary, it would be a most remarkable coincidence if he did not draw one. The same event—the drawing of a prize—which in one case must be regarded as highly improbable, becomes in the other case highly probable. So it is with coincidences which appear utterly improbable. It would be a most wonderful thing if such coincidences did not occur, and occur pretty frequently, in the experience of every man, since the opportunities for their occurrence enormously outnumber the chances against the occurrence of any particular instance.

We may reason in like manner as to superstitions. Or rather, it is to be noted that the coincidences on which superstitions are commonly based are in many instances not even remarkable. Misfortunes are not so uncommon, for instance, that the occurrence of a disaster of some sort after the spilling of salt at table can be regarded as surprising. If three or four persons, who are discussing the particular superstition relating to salt-cellars, can cite instances of an apparent connection between a misfortune and the contact of salt with a table-cloth, the circumstance is in no sense to be wondered at; it would be much more remarkable if the contrary were the case. There is scarcely a superstition of the commoner sort which is not in like manner based, *not* on some remarkable coincidence, but on the occasional occurrence of quite common coincidences. It may be said, indeed, of the facts on which nearly all the vulgar superstitions have been

based, that it would have amounted to little less than a miracle if such facts were not common in the experience of every person. Any other superstitions could be just as readily started, and be very quickly supported by as convincing evidence. If I were to announce to-morrow in all the papers and on every wall that misfortune is sure to follow when any person is ill-advised enough to pare a finger-nail between ten and eleven o'clock on any Friday morning, that announcement would be supported within a week by evidence of the most striking kind. In less than a month it would be an established superstition. If this appears absurd and incredible, let the reader consider merely the absurdity of ordinary superstitions. Take, for instance, fortune-telling by means of cards. If our police reports did not assure us that such vaticination is believed in by many, would it be credible that reasoning beings could hope to learn anything of the future from the order in which a few pieces of painted paper happened to fall when shuffled? Yet it is easy to see why this or any way of telling fortunes is believed in. Many persons believe in the predictions of fortune-tellers for the seemingly excellent reason that such predictions are repeatedly fulfilled. They do not notice that (setting apart happy guesses based on known facts) there would have been as many fulfilments if every prediction had been precisely reversed. It is the same with other common superstitions. Reverse them, and they are as trustworthy as before. Let the superstition be that to every one spilling salt at dinner some great

piece of good luck will occur before the day is over; let seven years of good fortune be promised to the person who breaks a mirror; and so on: these new superstitions would be before long supported by as good evidence as those now in existence; and they would be worth as much—since neither would be worth anything.

NOTES ON POKER.

The existence and still more the flourishing condition of such a game as poker, outside mere gambling-dens, is one of the most portentous phenomena of American civilisation, though it is not in this aspect that I propose just now to consider it; for the art which chiefly avails to help the gambler in playing this game is nothing more nor less than that art of which the enemy of man is proverbially said to be the father. Poker has an advantage over whist in one respect. In whist skill will do somewhat; but it will not avail to make good cards yield to bad ones. In poker the case is otherwise. A man shall have not a point in his hand; yet by sheer bluffing—in other words, by lying—he shall cause such an idea to be formed of his hand, that every one else at the table will throw up his cards, and leave to the liar full possession of the stakes. Yet, as Lawrence in 'Guy Livingstone,' and Hawley Smart in half a dozen novels, describe with approval the success of daring swindles, so the enthusiastic poker-player will tell you with pride of achievements in bluffing which can only be viewed in one way by men of honour—to wit, as barefaced lying.

The game of poker is sufficiently simple, though, as usual, the explanation given by those who play it is obscure in the extreme. To every one in the circle five cards are dealt in the usual way. The eldest hand—*i.e.* the player next the dealer on the left—stakes a sum, which must be doubled by all who intend to stay in; the eldest hand doubling his original stake if he decides to stay in, otherwise forfeiting it. When this is done all who stay in have staked an equal sum. Each player may (in his regular turn only) increase his stake, in which case all who wish to stay must 'see' him—that is, raise their stake in the same degree, or go better—that is, raise the stake further. When all are equally in, each of the players can throw out any of his cards, and draw as many more, to improve his hand. This done, the real business begins. In due rotation the players left in raise the stake, or follow in 'seeing' it—that is, in bringing up their stakes to the increased value. This may go on, and generally does go on, till each has staked a large sum. If a sum is named which a player is unwilling to 'see,' he lays down his hand. If all the other players are unwilling to 'see' a bet, they all throw down their hands, and the bettor takes the pool without showing his hand. But when the bet goes round to the last player remaining in, and he does not wish to go better, he may simply 'see it' and 'call'; on which all playing must show their hands, and the best hand wins the pool.

On the rules which determine the value of the several hands depend whatever qualities the game of

poker has as a game of skill. Just as in *vingt-et-un*, hazard, and like games, there are certain rules of probability which ought to guide the player (if he must gamble), so also in poker there are rules, though they very little affect the play of the average poker-player, while the really skilled professors of this cheerful game pay no attention to them whatever.

The points which give a hand value are the presence of cards of the same denomination (as a *pair*, or two of the same denomination; *triplets*, or three of a kind; and *fours*, or four of a kind); a *sequence*—that is, all the cards in the hand being in sequence, as 9, 10, knave, queen, king; a *flush*, or all the cards of the same suit. The lowest kind of hand is one which has none of these points; such a hand is estimated against others of the same kind by the highest card in it (the value of the cards being as in whist). Next in value is a hand with one pair in it; next a hand with two pairs (different pairs, of course); next a hand with three cards of the same denomination, called 'threes'; next a sequence hand; next a flush hand; then a *full* hand—that is, a hand containing one pair and one triplet; then *fours*, a hand containing four cards of the same denomination; and, lastly, that is highest and best of all, a *flush sequence*—that is, a sequence of high cards all of the same suit. In every case where two hands are of the same kind, the cards of highest denomination in the pair, triplet, four, flush, or sequence, wins. Thus a flush sequence of knave, 10, 9, 8, 7, beats a flush sequence of 9, 8, 7, 6, 5; four aces beat four kings or four queens;

a full of three aces and two deuces beats a full of three kings and two queens, but a full of three aces and two threes beats a full of three aces and two deuces; a flush of king, 7, 5, 3, 2, beats a flush of queen, knave, 10, 9, 7; and so on. In cases of 'tie 'th stakes are divided.

It is clear that the game itself is as good as many which are played in the domestic circle. In such a game as *vingt-et-un*, for instance, where the players are all against the dealer, there is about the same element of chance and about the same room for the exercise of judgment that there is in a game of poker which is to end with a call. But the bluffing element, which is what gives the game its real value to the gambling fraternity, is independent of any qualities possessed by poker as a card game. Where there is no 'limit' (that is, no stated sum beyond which no bet must go), one can bluff as well, and almost as safely, over a bad hand as over a good one—if one possesses the requisite qualities of a false face and a steady nerve.

But I wish just now to consider the qualities which this game possesses as an exercise of the judgment. No judgment is shown by one who sits down to gamble at poker; but in the game itself there are points depending a good deal on judgment, and especially on a knowledge of the laws of chance. Here, oddly enough, the professional poker-players have made, for the most part, little progress. We have before us the reasoning of one who claims to teach, calling his book 'The Complete Poker Player,' and we find not only much that is

incorrect in theory, but an absolute failure to understand the real value of the principles of probability to the poker proficient, and indeed to all who gamble. He deliberately tells us, in fact, that while theory shows the odds to be such and such, experience points to other odds, the real fact being that experience and theory are in most perfect accord in all matters of probabilities.

In the first place, the problems connected with the decision, whether to stay in or retire on a given hand, are very pretty. The case is entirely different from that to be dealt with in such a game as *vingt-et-un*, where only the dealer has to be considered, each player being as it were in contest with him. In poker a player has to consider, not the chance of having a better hand than some particular adversary, but the chance that he holds better cards than *any* of the others. This modifies the chances in a very interesting manner. Not only are they different from those existing where each player is matched against the dealer, but they vary according to the number of players. Where the players are few a moderately good hand may be trusted to win against the company, in the average of a great number of trials; but where there are many players there is more chance of a strong hand lying somewhere to beat it, and therefore, the hand in which the player should decide to trust must be a better one. For instance, with few players a pokerist might safely decide that he would not go in on less than a high pair, as kings or aces, and adhering to that rule throughout the play would be likely to come out without heavy loss. But if there

were a large party of players, the average best hand at each deal would probably be better; and he might, therefore, deem it well to put low threes, as three fours or three fives, as the limit below which he would not back his hand. Apart from 'bluffing,' such rules are not affected by the probability that a 'call' may be made; for the persistence of other players in raising will depend on the quality of their hand.

But we touch here on a characteristic of this game of poker, which makes it a really excellent game for non-gamblers, because calling so largely on the exercise of judgment, and also depending so much on individual character. As a parlour game, with counters instead of coin, it is one of the best and most amusing I know of. It is strangely contrasted with whist, calling for the exercise of very different mental faculties, but bringing out traits of character in quite as marked a degree.

As a result of confidence in luck, either general or at any particular time, poker-players often trust in hands of far less value than such as would give a fair chance of winning. It never seems to occur to them that the possession of a bad hand should in itself be regarded, if the theory of luck were sound, as an evidence that at the moment they were not in the vein; and that the principle 'back your luck' would suggest that the hand should be thrown up, for backing it means backing bad luck.

Of course this does not apply to bluffing, which, however, is not considered good poker-playing, at least as a system. A player may bluff on almost any hand,

and the bolder his bluff the better his chance of winning; for his opponent has to pay to see his hand—he has, indeed, in a sense, not to pay but simply to stake so much money; but, according to the true doctrine of chances, staking means payment of a certain sum for a certain chance. Now, when a poker-player raises the stakes by a very large amount, he means, if he is not bluffing, 'I have a very good hand;' and it is not wise, if that is the case, to pay a large sum for the privilege of seeing how good his hand is, unless your own is so good as to give you a very good chance of having the better. Even then it is better to see and go better than to call. For by so doing you have two chances to one —the chance that, seeing you so confident, he will not go on, and the chance that when the call is made you will be found to have the better hand. Now, a bold bluff often forces success—*if the player is not given to bluffing*. If he is, he is soon found out; and thereafter he bluffs at his proper peril. Probably no bluffing poker-player has ever been successful for any great length of time. Even if he is so wealthy that he can stand a few checks so far as his pocket is concerned, he begins to lose nerve when a few large bluffs have been met with a call and his pockets have suffered accordingly. But the player who nine times out of ten plays the straight game, may often win largely by an occasional bluff—if he is ready to overlook the fact that a bluff is a lie.

But the avoidance of bluffing takes away none of the good qualities which poker has as a game of skill.

The player may still back his hand with more or less boldness, according to its quality and his temperament. He still requires to exercise judgment as to the actual or relative value of a hand; he still has to note observantly what is done by other players, what cards they draw, what their ways are in standing on a hand, in holding when advances are made by others, and so forth.

In actual play for money the use of a good limit below which the player makes it a rule to stand out is sound policy; for in the long run the player whose lowest hand for backing is a strong one, as two aces, or low threes at the least in small companies, and high threes in large companies, must come off well. He will win more than he loses. But it must be remembered that constant caution is apt to diminish the profits of successful ventures. The poker-player wants others to play high when he has a winning hand, and if it becomes known that he never backs any but strong hands, none will 'raise' very much against him. To succeed in pocketing a large share of other people's money, which is the true poker-player's object, the most cautious player must indulge in an occasional extravagance. So also with a very strong hand—one that is practically sure to win—the judicious poker-player must play a waiting game. He must reverse the tactics of the bluffer, who tries to persuade others that his hand is better than it really is; he must try to persuade the rest that his hand is but a poor one; so will they see and raise, see and raise, until there is something in the pool worth winning, when he can see and raise more

boldly, and finally call or await the call with confidence. (In fact, lying and lying in wait are the secrets of success at poker.)

Let us consider briefly what are the chances for each different kind of hand at poker.

First, the total number of ways in which a set of five cards can be formed out of a pack containing 52 cards has to be determined. This is easy enough. You multiply together 52, 51, 50, 49, and 48, and divide the product by that obtained from multiplying together 1, 2, 3, 4, and 5. You thus get 2,598,960 as the total number of poker hands.

It is very easy to determine the number of flushes and sequences and flush sequences which are possible.

Thus, begin with the flush sequences. We can have in each suit, Ace, 2, 3, 4, 5 ; 2, 3, 4, 5, 6 ; 3, 4, 5, 6, 7 ; and so on up to 10, Knave, Queen, King, Ace ; or in all there are ten flush sequences in each suit, forty flush sequences in all.

The number of sequences which are not flush may be thus determined. The arrangement of numbers may be any one of the ten just indicated. But taking any one of these, as 3, 4, 5, 6, 7, the three may be of any suit out of the four ; so that each arrangement may be obtained in four different ways as respects the first card ; so with the second, third, &c. ; or in all 4 times 4 times 4 times 4 times 4, or 1,024, four of which only will be flushes. Thus there are 1,020 times 10, or 10,200 sequences which are not flush.

Now as respects flushes their number is very easily

determined. The number of combinations of five cards which can be formed out of the 13 cards of a suit are given by multiplying together 13, 12, 11, 10, and 9, and dividing by the product of 1, 2, 3, 4, 5; this will be found to be 1,287. Thus there are 4 times 1,287, or 5,148 possible flushes. Of these 5,108 are not sequence flushes.

The total number of 'four' hands may be considered next. The process for finding it is very simple. There are of course only 13 fours, each of which can be taken with any one of the remaining 48 cards; so that there are 13 times 48, or 624 possible four hands.

Next, to determine the number of 'full hands.' This is not difficult, but requires a little more attention. A full hand consists of a triplet and a pair. Now manifestly there are four triplets of each kind—four sets of three aces, four of three kings, and so forth (for we may take each ace from the four aces in succession, leaving in each case a different triplet of aces; and so with the other denominations). Thus, in all, 4 times 13, or 52 different triplets can be formed out of the pack of 52 cards. When one of these triplets has been formed there remain 49 cards, out of which the total number of sets of two which can be formed is obtained by multiplying 49 by 48 and dividing by two; whence we get 1,176 such combinations in all. But the total number of pairs which can be formed from among these 49 cards is much smaller. There are four twos, which (as cribbage teaches us) will give six pairs of twos; so there are six pairs of threes, six pairs of fours, and so on; or

as there are only twelve possible kinds of pairs (after our triplet is removed) there are in all 6 times 12, that is 72, possible pairs which can with the triplet form a full hand. Hence, as there are 52 possible triplets, the total number of full hands is 52 times 72, or 3,744.

The number of triplet hands which are not also fours or fulls (for every four hand contains triplets) follows at once from the above. There are 52 possible triplets, each of which can be combined with 1,176 combinations of two cards out of the remaining 49, giving in all 52 times 1,176, or 61,152 sets of five, three at least of which are alike. But there are 624 four hands, each of which is not only a triplet hand but will manifestly make four of the triplet hands our gross reckoning includes (for from every four you can make three triplets), and there are 3,744 full hands. These (to wit 2,496 fours and 3,744 fulls, or 6,240 hands in all) must be removed from our count, leaving 54,912 triplet hands (proper) in all.

This last result might have been obtained another way, which (as I shall use it for counting pair hands) I may as well indicate here. Taking any triplet of the 52 there remain 49 cards, one of which is of the same denomination as the triplet. Removing this, there are left 48 cards, out of which the number of sets of two which can be formed is obtained by multiplying 48 by 47 and dividing by 2; it is therefore 1,128, and among these 72 are pairs. There remain then 1,056 sets of two, any one of which can be combined with each of

52 triplets to give a triplet hand pure and simple. Thus, in all, there are 52 times 1,056 triplet hands, or 54,912, as before.

Next for double and single pairs.

From the whole pack of 52 cards we can form six times 13 pairs; for 6 aces can be formed, 6 pairs of deuces, 6 pairs of threes, and so forth. Thus there are in all 78 different pairs. When we have taken out any pair, there remain 50 cards. From these we must remove the two cards of the same denomination, as either or both of these must not appear in the hand to be formed. There remain 48 cards, from which we can form 72 other pairs. Each of these can be taken with any one of the 46 remaining cards, except with those two which are of the same denomination, or with 44 in all, without forming a triplet. Each of these combinations can be taken with each of the 78 pairs, giving a two-pair hand, only it is obvious that each two-pair hand will be given twice by this arrangement. Thus the total number of two-pair hands is half of 78 times 72 times 44; or there are 123,552 such hands in all.

Next, as to simple pairs. We get, as before, 78 different pairs. Each of these can be taken with any set of three formed out of the 48 cards left when the other 2 of the same denomination have been removed, except the 72 times 44 (that is 3,168) pairs indicated in dealing with the last case, and the 48 triplets which can be formed out of these same 48 cards, or 3,216 sets in all. Now the total number of sets of three cards which can be formed out of 48 is given by multiplying

48 by 47 by 46, and dividing by the product of the numbers 1, 2, and 3. It is found to be 17,296. We diminish this by 3,216, getting 14,082, and find that there are in all 78 times 14,082 or 1,098,240.

The hands which remain are those which are to be estimated by the highest card in them; and their number will of course be obtained by subtracting the sum of the numbers already obtained from the total number of possible hands. We thus obtain the number 1,302,540.

Thus of the four best classes of hands, there are the following numbers:

Of flush sequences there may be . . .	40
„ fours	624
„ full hands	3,744
„ common flushes	5,108
„ common sequences	10,200
„ triplets	54,912
„ two pairs	123,552
„ pairs	1,098,240
„ other hands[1]	1,302,540
Total number of possible hands . .	2,598,960

[1] It is easy to test the accuracy of the whole series of calculations by determining independently how many hands there are which do not belong to the first eight classes. Thus, as all the cards of the five are of different denominations, we first take the combinations of the thirteen card names five together. These (as in dealing with common flushes above) are 1,287 in number. But, as in dealing with common sequences, we must multiply these by 4 times 4 times 4 times 4 times 4, or by 1,024, getting 1,317,888. Subtracting thence the flushes and sequences, 15,348 in all, we get 1,302,540 as the total number of common hands (not containing pairs or the like)—as above.

It will be seen that those who devised the rules for poker play set the different hands in their proper order. It is fitting, for instance, that as there are only 40 possible flush sequence hands, out of a total number of 2,598,960 hands, while there are 624 'four' hands, the flush sequences should come first, and so with the rest. It is noteworthy, however, that when sequences were not counted, as was the rule in former times, there was one hand absolutely unique and unconquerable. The holder of four aces then wagered on a certainty, for no one else could hold that hand. At present there is no absolutely sure winning hand. The holder of ace, king, queen, knave, ten, flush, *may* (though it is of course exceedingly unlikely) be met by the holder of the same cards, flush, in another suit. Or, when we remember that at whist it *has* happened that the deal divided the four suits among the four players, to each a complete suit, we see that four players at poker *might* each receive a flush sequence headed by the ace. Thus the use of sequences has saved poker-players from the possible risk of having either to stand out or wager on a certainty, which last would of course be very painful to the feelings of a professional gambler.

We might subdivide the hands above classified into a much longer array, beginning thus :—4 flush sequences headed by ace; 4 headed by king, and so on down to 4 headed by five; 48 possible four-aces hands; 48 four-kings hands; and so on down to 48 four-twos hands; 24 possible 'fulls' of 3 aces and 2 kings; as many of 3 aces and 2 queens; and so on down to 24 'fulls' of 3

twos and 2 threes; and so on. Any one who cares to do this can, by drawing the line at any hand, ascertain at once the number of hands above and not above that hand in value: and thus determine the chance that any hand taken at random is above or below that particular hand in value. The comparatively simple table above only shows how many hands there are above or not above pairs, triplets, and the like. But the more complete series could be very easily formed.

We note from the above table that more than half the possible poker hands are below pairs in value. So that Clay was right enough in wagering on an ace-high hand, seeing that there are more hands which will not beat it (supposing the highest next card a king, at any rate) than there are hands that will; but he was quite wrong in calling on such a hand, even against a single opponent.

The effect of increase in the number of hands can also readily be determined. Many even among gamblers know so little of the doctrine of chances as not to be aware of, still less to be able to measure the effect of, the presence of a great number of other contestants. Yet it is easy to illustrate the matter.

Thus, suppose a player casts a die single against one other. If the first has cast four the odds are in favour of his not being beaten; for there are only two casts which *will* beat him and four which will not. The chance that he will not be beaten by a single opponent is thus $\frac{4}{6}$ths or $\frac{2}{3}$. If there is another opponent, the chance that he individually will not cast better than 4,

is also $\frac{2}{3}$. But the chance that neither will throw better than 4 is obtained by multiplying $\frac{2}{3}$ by $\frac{2}{3}$. It is therefore $\frac{4}{9}$; or the odds are 5 to 4 in favour of one or other beating the cast of the first thrower. If there are three others, in like manner the chance that not one of the three will throw better than 4 is obtained by multiplying $\frac{2}{3}$ by $\frac{2}{3}$ by $\frac{2}{3}$. It is therefore $\frac{8}{27}$; or the odds are 19 to 8 in favour of the first thrower's cast of 4 being beaten. And so with every increase in the number of other throwers, the chance of the first thrower's cast being beaten is increased. So that if the first thrower casts 4, and is offered his share of the stakes before the next throw is made, the offer is a bad one if there is but one opponent, a good one if there are two, and a very good one if there are more than two.

In like manner, the same hand which it would be safe to stand on (as a rule) at poker against two or three opponents, may be a very unsafe hand to stand on against five or six.

Then the player has to consider the pretty chance-problems involved in drawing.

Suppose, for instance, your original hand contains a pair—the other three cards being all unlike: should you stand out? or should you draw? (to purchase right to which you must stand in); or should you stand in without drawing? Again, if you draw, how many of the three loose cards should you throw out? and what are your chances of improving your hand?

Here you have to consider first whether you will

stand in, which depends, not on the value of your pair only, but also on the chance that your hand will be improved by drawing. Having decided to stand in, remember that discarding three tells the rest of the company that in all possibility you are drawing to improve a pair hand; and at poker, telling anything helps the enemy. If one of your loose cards is an ace, you do well to discard only the other two; for this looks like drawing to a triplet, and you may chance to draw a pair to your ace. But usually you have so much better a chance of improving your hand by drawing three that it is, as a rule, better to do this.

Drawing to a triplet is usually good policy. 'Your mathematical expectation of improvement is slight,' says 'The Complete Poker Player,' 'being 1 to 23 of a fourth card' (it should be *the* fourth card) 'of the same denomination, and 2 to 23 of another pair of denomination different from the triplet,' a remark suggesting the comment that to obtain a pair of the same denomination as the triplet would require play something like what we hear of in old Mississippi stories, where a 'straight flush' would be met by a very full pair of hands, to wit, five in one hand and a revolver in the other! The total expectation of improvement is 1 to 8; but then see what an impression you make by a draw which means a good hand. Then, too, you may suggest a yet better hand, without much impairing your chance of improvement, by drawing one card only. This gives you one chance in 47 of making fours, and 1 in 16 of picking up one of the three cards of the same

denomination as the odd cards you retain. This is a chance of 1 in 12.

'Draws to straights and flushes are usually dearly purchased,' says our oracle; 'always so at a small table. Their value increases directly as the number of players.' (The word 'directly' is here incorrectly used; the value increases as the number of players, but not *directly* as the number.) Of course in drawing to a two-ended straight, that is one which does not begin or end with an ace, the chance of success is represented by 8 in 47, for there are 47 cards outside your original hand of which only eight are good to complete the straight. For a one-end straight the chance is but 4 in 47: with a small chance, too, of improving your hand, you are trying for a hand better than you want in any but a large company. 'If you play in a large party,' says 'The Complete Poker Player,' 'say seven or eight, and find occasion to draw for a straight against six players, do so by all means, even if you split aces.' The advice is sound. Under the circumstances you need a better hand than ace-pair to give you your fair sixth share of the chances.

As to flushes your chances are better, when you have already four of a suit. You discard one, and out of the remaining 47 cards any one of nine will make your flush for you. Your chance then is 1 in $5\frac{2}{9}$. In dealing with this point our oracle goes altogether wrong, and adopts a principle so inconsistent with the doctrine of probabilities as to show that, though he knows much more than Steinmetz, he still labours under somewhat

similar illusions. 'Theoretically,' says he, 'the result just obtained is absolutely true; but I have experimented with six hands through a succession of 500 deals, and filled only 83 flushes in the 500, equal to one in six and one-twentieth draws. Of course I am not prepared to say that this would be the average in many thousand deals; theoretically it is an untrue result; but I here suggest a *possible* explanation of what I confess is to me a mystery.' Then he expounds the very matter on which we touched above. 'In casting dice,' he says, '*theoretically*, any given throw has no influence upon the next throw, and is not influenced by the previous throw. Yet if you throw a die and it turns up six, while the chances are *theoretically* one to six' (one in six it should be) 'that the next throw will produce a six because the previous throw of six lies absolutely in the past, yet you may safely bet something more than the usual odds against it. Then suppose the second throw turns up a six, that throw also now lies in the past, and cannot be proved to have an influence upon throw number three, which you are preparing to make. If any *material* influence is suspected you may change the box and die; and you may now bet twice the usual odds against the six. Why? Because you know by experience that it is extremely difficult to throw six three times in succession, even if you do not know the precise odds against it. Granted certain odds against throwing six twice in succession, &c., yet at any given moment when the player shakes the box in which is a six-faced die, he has one chance in six of throwing

a six; and yet if he has just thrown sixes twice, you may bet twelve to one that he will not throw a six in that particular cast.' If I did not hold gambling to be near akin to swindling, and could find but a few hundred who held this doctrine, how much money might I not gain by accepting any number of wagers of this wise sort!

The fact is, the mistake here is just the ridiculous mistake which Steinmetz called 'the maturity of the chances,' over again. It is a mistake which has misled to their ruin many thousands of gamblers, who might have escaped the evil influence of that other equally foolish mistake about being lucky or unlucky, in the vein or out of it. Steinmetz puts the matter thus:—'In a game of chance, the oftener the same combination has occurred in succession, the nearer are we to the certainty that it will not recur at the next cast or turn up: this is the most elementary of the theories on probabilities; it is termed the maturity of the chances.' The real fact being that this is not a theory of probabilities at all, but disproved by the theory of probabilities—and disproved, whenever it has been put to the test, by facts.

Take the case considered in 'The Complete Poker Player,' and note the evidence on the strength of which the author of that work rejects the theory in favour of a practical common-sense notion (as he thinks), which is, in reality, nonsense. You may expect 9 successful draws to a flush in 47 hands; therefore, in the 500 deals he experimented upon, he might have expected 95 or 96; and he only obtained 83. Now 500 trials are far too

few to test such a matter as this. You can hardly test even the tossing of a coin properly by fewer than a thousand trials; and in that case there are but 2 possible events. Here there are 47, of which 9 are favourable. It was the failure to recognise this which led the Astronomer-Royal for Scotland to recognise something mystical and significant in the preponderance of 3's and the deficiency of 7's among the digits representing the proportion of the circumference to the diameter of a circle. In casting a coin a great number of times, we do not find that the occurrence of a great number of successive heads or tails in any way affects the average proportion of heads or tails coming next after the series. Thus I have before me the record of a series of 16,317 tossings, in which the number of sequences of tails (only) were rendered; and I find that after 271 cases in which tails had been tossed 5 times in succession, the next tossing gave in 132 cases heads, and in 139 cases tails. Among the 16,317 tossings, two cases occurred in which tail was tossed 15 times in succession.

MARTINGALES; OR, SURE (?) GAMBLING SYSTEMS.

In previous pages I have considered, under the head of 'Gamblers' Fallacies,' certain plans by which some fondly imagine that fortune may be forced. I have shown how illusory the schemes really are which at first view appear so promising. There are other plans the fallacy in which cannot be quite so readily seen, though in reality unmistakable, when once the conditions of the problem are duly considered.

Let me in the first place briefly run through the reasoning relating to one of the simpler methods already considered at length.

The simplest method for winning constantly at any such game as *rouge-et-noir* is as follows:—The player stakes the sum which he desires to win, say 1*l.* Either he wins or loses. If he wins he again stakes 1*l.*, having already gained one. If, however, he loses, he stakes 2*l.* If this time he wins, he gains a balance of 1*l.*, and begins again, staking 1*l.*, having already won 1*l.* If, however, he loses the stake of 2*l.*, or 3*l.* in all (for 1*l.* was lost at the first trial), he stakes 4*l.* If he wins at this third trial, he is 1*l.* to the good, and

begins again, staking 1*l.* after having already won 1*l.* If, however, he loses, he stakes 8*l.* It will readily be seen that by going on in this way the player always wins 1*l.* when at last the right colour appears. He then, in every case, puts by the 1*l.* gained and begins again.

It seems then at first as though all the player has to do is to keep on patiently in this way, starting always with some small sum which he desires to win at each trial, doubling the stake after each loss, when he pockets the amount of his first stake and begins again. At each trial the same sum seems certainly to be gained, for he cannot go on losing for ever. So that he may keep on adding pound to pound, *ad infinitum*, or until the 'bank' tires of the losing game.

The fallacy consists in the assumption that he cannot always lose. It is true that theoretically a time must always come when the right colour wins. But the player has to keep on doubling his stake practically, not theoretically; and the right colour may not appear till his pockets are cleared. Theoretically, too, it is certain that be the sum at his command ever so large, and the stake the bank allows ever so great, the player will be ruined at last at this game, if—which is always the case—the sum at the command of the bank is very much larger. It would be so even if the bank allowed itself no advantage in the game, whereas we know that there is a certain seemingly small, but in reality decisive, advantage in favour of the bank at every trial. Apart from this, however, the longest pocket is bound to win in the long run, at the game of speculation

which I have described. For, though it seems a tolerably sure game, it is in reality purely speculative. At every trial there is an enormous probability in favour of the player winning a certain insignificant sum; but, *per contra*, there is a certain small probability that he will lose, not a small sum, or even a large sum, but all that he possesses—supposing, that is, that he continues the game with steady courage up to that final doubling which closes his gambling career, and also supposing that the bank allows the doubling to continue far enough; if the bank does not, then the last sum staked within the bank limit is the amount lost by the player, and, though he may not be absolutely ruined, he loses at one fell swoop a sum very much larger than that insignificant amount which is all he can win at each trial.

Although this gambling superstition has misled many, yet after all it is easily shown to be a fallacy. It is too simple to mislead any reasonable person long. And indeed, when it has been tried, we find that the unfortunate victim of the delusion very soon wakes to the fact that his stakes increase dangerously fast. When it comes to the fifth or sixth doubling, he is apt to lose heart, fearing that the luck which has gone against him five times in succession may go against him five times more, which would mean that the stake already multiplied 32 times would be increased, not 32 times, but 32 times 32 times, or 1,024 times, which would either mean ruin or a sudden foreclosure on the bank's part and the collapse of the system.

For the benefit of those who too readily see through a simple scheme such as this, gamblers have invented other devices for their own or others' destruction, devices in which the fallacy underlying all such plans is so carefully hidden that it cannot very readily be detected.

The following is a martingale (as gamblers call these devices for preventing fortune from rearing against them) which has misled many:

The gambler[1] first decides on the amount which he is to win at each venture—if that can be called a venture which according to his scheme is to be regarded as an absolute certainty. Let us say that the sum to be won is 10*l*. He divides this up into any convenient number of parts, say three; and say that the three sums making up 10*l*. are 3*l*., 3*l*., and 4*l*. Then he prepares a card on the annexed plan (fig. 1), where W stands for winnings, L for losses, and M (for martingale) heads the working column which guides the gambler in his successive ventures.

W	M	L
	£3	
	3	
	4	

FIG. .

The first part of the play is light and fanciful: the player—whom we will call A—stakes any small sums he pleases until he loses, making no account of any winnings which may precede his first loss. This first loss starts his actual operations. Say the first loss

[1] The account of the system here considered appeared in the *Cornhill Magazine* under the heading 'A San Carlo Superstition,' and was in that place described as 'a pretty little martingale' recently submitted to me by a correspondent of *Knowledge*.

amounts to 2*l.*: A enters this sum in the third column (see fig. 2) as a loss, and also in the second under the cross-line. He then stakes the sum of this number, 2, which is now the lowest in column M, and 3, the uppermost—that is, he stakes 5*l.* If he loses, he enters the lost 5*l.* in columns M and L; and next stakes 8*l.*, the sum of the top and bottom figures (3*l.* and 5*l.*) in column M. He goes on thus till he wins, when he enters under the head W the amount he has won, and scores out in column M the top and bottom figures—viz., the 3*l.* (at the top), and the last loss (at the bottom). This process is to be continued, the last stake, if it be lost, being always scored at the bottom of column M, as well as in the loss column, the last win being always followed by the scoring out of the top and bottom remaining numbers in column M. When this process has continued until all the numbers in column M are scored out, A will be found to have won 10*l.*; and whatever the sum he had set himself to win in the first instance, so long as it lies well within the tolerably wide limits allowed by the bank, A will always win just this sum in each operation.

Let us take a few illustrative cases, for in these matters an abstract description can never be so clear as the account of some actual case.

Consider, then, the accompanying account by A of one of these little operations. The amount which A sets out to win is, as before, 10*l.* He divides this up into three parts—3*l.*, 3*l.*, and 4*l.* He starts with a loss of 2*l.*, which he sets in columns M and L. He stakes next 5*l.* and loses, setting down 5*l.* in columns M and L. He

stakes 8*l*., the sum of the top and bottom numbers in column M, and wins. He therefore sets 8*l*. under W, and scores out 3*l*. and 5*l*., the top and bottom numbers in column M. (The reader should here score out these numbers in pencil.) The top and bottom numbers now remaining are 3*l*. and 2*l*. Therefore A stakes now 5*l*. Say he loses. He therefore sets down 5*l*. both in column M and column L, and stakes 8*l*., the sum of the top and bottom numbers under M. Say he loses again. He therefore puts down 8*l*. under columns M and L, and stakes 11*l*., the sum of the top and bottom numbers under M. Say he wins. He puts down 11*l*. under W, and scores out the 3*l*. left at the top and the 8*l*. left at the bottom of the column under M. (This the reader should do in pencil.) He then stakes 9*l*., the sum of the top and bottom numbers (4*l*. and 5*l*. respectively) left under M. Say he wins again. He then puts down 9*l*. under W, and scores out the 4*l*. left at the top and the 5*l*. left at the bottom of the column under M. There now remains only one number under M, namely, 2*l*., and therefore A stakes 2*l*. Let us suppose that he loses. He puts down 2*l*. under M and L, and, following the simple rule, stakes 4*l*. Say he wins. He then puts down 4*l*. under W, and scores out 2*l*. and 2*l*., the only two remaining numbers under M. A, therefore, now closes his little account, finding himself the winner of

W	M	L
	£3	
	3	
	4	
	2	£2
	5	5
£8	5	5
11	8	8
9	2	2
4		
£32		£22

FIG. 2.

8*l.*, 11*l.*, 9*l.*, and 4*l.*, or 32*l.* in all, and the loser of 2*l.*, 5*l.*, 5*l.*, 8*l.*, and 2*l.*, or 22*l.* in all, the balance in his favour being 10*l.*, the sum he set forth to win.

It seems obvious that the repetition of such a process as this, any convenient number of times at each sitting, must result in putting into A's pocket a considerable number of the sums of money dealt with at each trial. In fact, it seems at a first view that here is a means of obtaining untold wealth, or at least of ruining any number of gambling-banks.

Again, at a first view, this method seems in all respects an immense improvement on the simpler one. For whereas in the latter only a small sum can be gained at each trial, while the sum staked increases after each failure in geometrical progression, in this second method (though it is equally a gambling superstition) a large sum may be gained at each trial, and the stakes only increase in arithmetical progression in each series of failures.

The comparison between the two plans comes out best when we take the sum to be won undivided, when also the system is simpler; and, further, the fallacy which underlies this, like *every* system for gaining money with certainty, is more readily detected, when we consider it thus.

Take, then, the sum of 10*l.*, and suppose 5*l.* the first loss, after which take two losses, one gain, one loss, and two gains. The table will be drawn up then as shown—with the balance of 10*l.*, according to the fatal success of this system.

On the other hand, take the other and simpler method, where we double the original stake after each failure. Then supposing the losses and gains to follow in the same succession as in the case just considered, note that the first gain closes the cycle. The table has the following simple form (counting three losses to begin with):

W	M	L
	£10	
	5	£5
	15	15
	25	25
£35	20	20
25		
15		
£75		£65

We see then at once the advantage in the simpler plan which counterbalances the chief disadvantage mentioned above. This disadvantage, the rapid increase of the sum staked, is undoubtedly serious; but, on the other hand, there is the important advantage that at the first success the sum originally staked is won; whereas, according to the other plan, every failure puts a step between the player and final success. It can readily be shown that this disadvantage in the less simple plan just balances the disadvantage in the simpler plan.

W	L
	£10
	20
£80	10
£80	£70

But now let us more particularly consider the probabilities for and against the player involved in the plan we are dealing with.

Note in the first place that the player works down the column under M from the top and bottom, taking off two figures at each success, and each figure adding one figure at the bottom after each failure. To get

then the number of figures scored out we must double the number of successes; to get the number added we take simply the number of failures, and the total number of sums under M is therefore the original number set under M, increased by the number of failures. He will therefore wipe out, as it were, the whole column, so soon as twice the number of successes either equals or exceeds by one the number of failures (including the first which starts the cycle). Manifestly the former sum will equal the latter, when the last win removes two numbers under M, and will exceed the latter by one when the last win removes only one number under M.

Underlying, then, the belief that this method is a certain way of increasing the gambler's store, there is the assumption that in the long run twice the number of successes will equal the number of failures, together with the number of sums originally placed under M, or with this number increased by unity. And this belief is sound; for according to the doctrine of probabilities, the number of successes—if the chances are originally equal—will in the long run differ from the number of failures by a number which, though it may perchance be great in itself, will certainly be very small compared with the total number of trials. So that twice the number of successes will differ very little relatively from *twice* the number of failures, when both numbers are large; and all that is required for our gambler's success is that twice the number of successes should equal *once* the number of failures, together with

a *small* number, viz. the number of sums originally set under M, or this number increased by unity. So that we may say the gambler is practically certain to win in the long run in any given trial.

In this respect the method we are now considering resembles the gambling superstition before examined. In that case also the gambler is sure to win in the long run, as he requires but a single success to wipe out the losses resulting from any number of failures. He is in that case sure to succeed very much sooner (on the average of a great number of trials) than in the latter.

But we remember that even in that case where success seems so assured, and where success in the long run—*granting the long run*—is absolutely certain, the system steadily followed out means not success but ruin. No matter what the limit which the bank rules may assign to the increase of the stakes, so long as there *is* a limit, and so long as the bank has a practically limitless control of money as compared with the player, he must eventually lose all that he possesses.

Hence we cannot assume that, because the method we are considering insures success in the long run, the gambler can win to any extent when the long run is not assured to him. Here lies the fallacy in this, as in all other methods, of binding fortune to the gambler's wheel. The player finds that he must win in the long run, and he never stops to inquire what run is actually allowed him. It may be a short run, or a fair run, or even a tolerably long run; but the question for him is, will it be long enough? And note that it is not only

the limitation which the bank may assign to the stakes which we have to consider : the gambler's possessions assign a limit, even though the bank may assign none.

Let us see, then, what prospect there is that in this, as in the other case, a run of bad luck may ruin the player —or rather, let us see whether it be the case that in this, as in the other system, patient perseverance in the system may not mean certain ruin, which ruin may indeed arrive at the very beginning of the confident gambler's career.

Instead of all but certainty of success in each single trial which exists in the simpler case, there is in the case we are considering but a high degree of probability. It is very much more likely than not that in a given trial the gambler will clear the stake which he has set himself to win. (This is why we so often hear strong expressions of faith in these systems : again and again we are told with open-mouthed expressions of wonder that a system of this sort must be infallible, because, says the narrator, I saw it tried over and over again, and always with success.) Granted that it is so ; ndeed, it would be a poor system which did not give the gambler an excellent chance of winning a small stake, in return for the risk, by no means evanescent, that he may lose a very large one.

Observe, now, how the chances for and against are balanced between the two systems. Suppose such a run of ill-luck as in the simpler system would mean absolute defeat, because of the rapid increase (by

doubling) of the sum staked by the gambler. Say, for instance, a bank allows no stake to exceed 1,000*l.*, so that ten doublings of a stake of 1*l.*, raising the stake to 1,024*l.*, would compel the gambler to stop, and leave him with all his accumulated losses, amounting to 1,023*l.* Now, take the case of a gambler trying the other system for a gain of 10*l.*, divided into three sums, 3*l.*, 3*l.*, and 4*l.* under column M, and suppose that after winning a number of times he unfortunately starts ten defeats in succession, his first loss having been 3*l.*; then his second loss was 6*l.*; the third, 9*l.*; the fourth, 12*l.*, and so on; the tenth being 30*l.* His total loss up to this point amounts only to 165*l.*, and is, therefore, much less serious than his loss would have been had he begun by staking 1*l.*, and doubled that sum nine times, losing ten times in all. Moreover, his next stake, according to the system, is only 33*l.*, which is well within the supposed limit of the bank. But, on the other hand, to carry on the system, he now has to go on until he has cleared off all the thirteen sums in the column under M. To do this he has to run the risk of several further runs of ill-luck against him, and it is by no means necessary that these should be long runs of luck for the score against him to become very heavy indeed. Be it noticed that at every win he scores off only a small portion of the balance against him, while every run of luck against him adds to that score heavily. And notice, moreover, that while on this system he does not quickly approach the limit which the bank may assign to stakes, he much more quickly encroaches on his own capital—a circum-

stance which is quite as seriously opposed to his chance of eventual success as the finality of the bank limit. So far as the carrying out of his system is concerned, it matters little whether he is obliged to stop the play on the system because his pockets are emptied, or because the bank will not allow him further to increase his stake.

Similar remarks apply to the following method, which has recently been suggested by another correspondent of 'Knowledge' as an improved system:

'My improvements,' he writes, 'consisted, first, in arranging that two players should play in concert, one staking persistently upon one colour while the other staked upon the other. A run of ill-luck to one would then be somewhat counterbalanced by the run of good luck to the other, while sometimes both would seem to be winners.

'Second, in staking the *sum* of the extreme figures in the guide-column only when the number of figures in it was even; when they were odd, *e.g.*,
1
2
3
4
5
only the highest, 5, is staked. Thus the rise of the stakes is considerably reduced, while the principle of the play is still carried out.

'Third, in splitting up a game when a run of ill luck has occurred into two or more games, and winning these *seriatim*. Suppose, for instance, that the chances of the game have brought the guide-column into the

form given in the margin. The player has actually lost 30, and must win 36 to gain 6. He might stake 36, but this would be rash. He should play more cautiously, and convert the column into 3 new columns, totalling 12 each, or even into 4, totalling 9,

1	
2	1
3 of 2	or 4 of 2
3	3
4	3
—	—
Total 12	Total 9

−1
−2
−3
3
−4
−4
6
6
−9
12

FIG. 3.

The numerals with a *minus* sign are supposed to be struck out.

and play out three or four encounters with the guidance of these columns. If luck makes the securing of success in these a long affair, his partner is meanwhile reaping the benefit of a run upon his colour.

'I believe that, allowing the bank its small advantage, the chance of winning 5 events out of 12, 6 out of 15, &c., is large. But, of course, the possible gain is small compared with the possible loss; and here, I have no doubt, the plan breaks down.'

The plan is only safer than the others in the sense that it prolongs the agony. The introduction of two partners does not affect the validity of the system one way or the other; for the chances of each must be considered separately, though their gains or losses are afterwards to be divided. The only point to be considered in that respect is the idea that the bad effects for one partner of a run on a colour would be corrected by the good effects for the other. As a matter of fact, there would be no such compensation. A run on one

colour which would set one of the partners two or three hundred pounds to the bad, would perhaps gain for the other forty or fifty pounds at the outside. Then it must be remembered that we not only have to consider the actual loss when an unfavourable colour appears, but its effect on the operation of the system. During an unfavourable run the stakes are rising and the distance to be covered before (if ever) safety is reached is increasing. By the suggested improvements the rate of increase in the stakes is undoubtedly diminished, but the rate at which the desired goal is approached is diminished in equivalent degree. I scarcely recommend any one to test any of these systems experimentally, even though without any idea of putting them into actual practice. It is easy enough to apply such a test by tossing a coin or cutting a pack a sufficient number of times. For, as the essential principle of all such systems is that they depend on the improbability of an event whose occurrence—when it does happen—will involve a heavy loss—a loss more than cancelling all preceding gains—it is naturally likely that any moderately long series of trials will seem to favour the theory, the fatal run not chancing to show in a series of trials too short to give it a fair chance of showing.

It has been thus indeed that many foolish folk have been tempted to trust in a system which has brought them to their ruin. Consider what an irony underlies the gambler's faith in such systems. When he starts with the hope of winning, say, 10*l.*, he is perhaps to

some degree doubtful; but he goes on until perhaps he is at such a stage that if he stopped he would be the loser of fifty or sixty pounds. Yet such is his confidence in his system that, although at this stage he is in a very much worse position than at the beginning, the mere circumstance that he is working out a system encourages him to persevere. And so he continues until the time comes—as with due patience and perseverance it inevitably must—when either the bank limit is reached or his pockets are emptied. In one case he has to begin again with a deficit against him much larger than any gain he has probably made before; in the second he has the pleasant satisfaction of noting, perhaps, that if he had been able to go on a little longer, fortune would (from his point of view) have changed. Though as a matter of fact, whether he had had a few hundreds of pounds more or not only affects his fortunes in putting off a little longer the inevitable day when the system fails and he is ruined.

We may compare the trust in a system to such trust as a bettor on races might put in laying long odds—when the odds are really long, but not quite so long as those he offers. Supposing a bettor to lay odds of 30 to 1 in sovereigns systematically, when the true odds are 25 to 1, he will probably win his sovereign on the average twenty-five times in twenty-six trials, but the 30*l.* he will have to pay in the twenty-sixth case (on the average) will leave him 5*l.* to the bad on that set of trials, excellent though his chance of success may appear at each separate trial.

In fine, the moths who seek to gain wealth rapidly and safely by gambling methods and systems are attracted almost equally by two equally delusive flames. They either trust in their own good luck, as in buying lottery tickets, backing the favourite, or the like, hoping to win large sums for small sums risked (these small sums, however, being always in excess of the just value of the chance); or they trust in the bad luck of others, as when they try delusive martingales (though they never see what they are really doing in such cases), or when they lay long odds (always longer than the just odds), hoping to win many small sums at small risk of losing large ones; or they combine both methods. Inevitably, in the long run, they lose more in many small sums than they get back in a few large ones; and they lose more in a few large sums than they get back in many small ones. They lose all round, yet they delude themselves all round into the belief that they are wise.

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